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Chapman's Lectures
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on
PHYSIOLOGY.

CONTENTS.

PHYSIOLOGY.

Physiology.

Lesson III.

Physiology.

Lecture 1st.

We are now to enter upon that section of our course which treats of the various functions of the animal economy or in other words of Physiology. To arrange all the parts of so complicated a structure in such a manner as to present a consistent explanation of the whole is a task of no little difficulty. My plan differs very materially from any of those which have been adopted by others. Commencing with an account of the process of Generation I shall in succession expatiate on those circumstances which relate to the foetal economy. This subject involves some of the most interesting speculations to which Physiology can give rise. As soon as the child has escaped from the womb of the mother it assumes a new and independent mode of existence. Next then I shall be led to enquire into the means by which it is nourished and supported and those by which it is connected with surrounding objects. First then let us direct our attention to the subject of Generation.

Generation. To this term different meanings are attached in the various sciences which constitute the vast mass of human knowledge. By Generation however, we understand that process by which the

human species is propagated, and thus limited, we shall now enter upon the discussion of it. It is peculiar to the living condition. Changes in dead matter are effected by laws contrary in their natural mode of operation. By Providence it has been ordained that all bodies shall be subject to the promiscuous hand of death, but the duration of life is diversified in the different classes of animals. Some live a century, others only a few years, months, or hours. Even Man, with all his attributes and faculties, is subject to the same unvarying and inexorable law; his body dies and decays like that of the meanest reptile. Exempt from this dismal destiny, his soul alone partakes in the immortality of its Creator. But though individuals perish, the race is preserved; the ranks are thinned by the ravages of Time, but new creatures step instantly into the places of the fallen. Deaths and births alternate in steady order. The moment that takes some away brings others into existence.

Generation is the greatest mystery in the animal economy; curious in every point of view, more particularly as it relates to ourselves, it has been the object of laborious investigation ever since the early ages. Notwithstanding the trouble which has been lavished upon it, we have still to regret the thick obscurity in which it is involved. The re-

=sult indeed, has been little more than the establishment of a few facts. But as is always the case when a sub=ject is imperfectly understood, an infinite number of futile hypotheses have been constructed and promul=gated, in a tone sufficiently confident and presump=tuous. Considering the varieties in structure, figure, and economy, in the vast chain of animated nature, it may easily be imagined how greatly diversified is the process of Generation. To trace all the varieties would occu=py more time than I can spare and besides would be en=croaching on the province of Natural History. My inten=tion therefore is to confine my observations to the process as it takes place in our own species referring occasion=ally to inferior animals and vegetables merely to borrow for illustration the striking examples they afford.

One of the few facts which have been indisputably settled is that the ovaries are the seat of conception as you will be taught by the Professor of Anatomy. Each of the ovaries contains a series of vesicles which are filled with a clear pellucid fluid. Of late it has been shewn that after fruitful coition one or more of these vesicles undergoes a change. The alteration consists in a gradual enlargement and loss of transparency an opaque and reddish hue being substituted for the transparency by which it was before characterized. After several sta=ges of maturation which are not inaptly compared to those of a small abscess the vesicle finally bursts and

discharges its contents which are received by the fim-
=briated extremities of the Fallopian tubes and conveyed
to the uterus there to be evolved and perfected. That
these are facts we have ample evidence. The ova in va-
=rious animals have been detected on their passage to
the uterus and in the human subject we sometimes
meet with extra-uterine conception in which the
foetus is either attached to some of the abdominal
viscera or found in the Fallopian tube or even in
the ovary itself. Nor is it less certain that the change
is effected by the fecundating influence of the semen
masculinum. But there is great difference of opinion
as to the precise mode in which this influence is
extended to the ovaries. By many it is maintained that
the semen is taken into the cavity of the uterus and
conveyed to the ovaries without any alteration in its pro-
=perties. In the early season of Physiology this hypothe-
=sis received the support of men engaged in medical
pursuits and owing to hasty and imperfect exami-
=nation has continued to the present day. My objection
to it will be confined to as small a space as possible.
The semen does not enter the uterine cavity and under
no circumstances does it ever penetrate so far as the
ovaries. Those who entertain a different opinion say in
general that the semen is injected into the uterus in
the act of coition. Other modes have been suggested. Aris-
=totle says "that the uterus sucks up the fecundating

halitus of the semen as scent is inhaled by the nostrils. Plato compares the uterus to an hungry animal which eagerly devours its food. Exceeding all in obscurity is the notion of a modern writer who says "that the os tincæ descends o=ver the glans penis exactly as a nightcap is pulled over the head." Dismissing without comment these silly flights of imagination let us return to the first hypothesis which possesses most intrinsic merit and is supported by the strong=est authority. That the male organ has the power of projec=tion is not denied for this is plainly evinced by the way in which the urine is discharged. When in the act of coition however this capacity is very much diminished if not wholly destroyed. Grasped tightly by the vagina the propelling muscles of the penis are cramped in their energies and unable to act as freely as when employed in propelling the urine. We also have in the tenacity of the semen another cause of resistance to its passage into the uterus. Besides the structure of the vagina instead of favouring is calculated by the rugæ on its surface to obstruct such a passage. But even admitting that it is projected as far as the uterus itself how will it be able to enter the cavity of that organ? It should be remembered that the opening of the os tincæ is as small as the ure=thra of the male and moreover is not in the axis of the vagina but inclining either forwards or backwards, to one side or the other. Nor is this all; the mouth of the uterus is filled with a glutinous substance capable of

resisting the entrance of the semen and when this is wanting the hard and unyielding lips of the os tincæ are nearly closed the passage in the neck is not much larger than a probe and that part of it and that part of it which is called the strait is still more contracted. Further obstructions are offered by the striæ and mucus which exist along the narrow canal. Besides the cavity of the uterus itself is so shallow that its surfaces are nearly in contact. What has hitherto been said refers to the parts in a healthy condition. Impediments arising from other morbid or congenital deficiencies have sometimes occurred which help to confirm the sentiment I have advanced. 1st. That the penis has had its powers of projection destroyed by various circumstances as by stricture tumefaction anomalous openings along the urethra by debility and relaxation. 2nd. The vagina has been closed by an adhesion of its sides by a membrane or by a tumour. 3rd. The os tincæ owing to original malformation or to inflammation has sometimes been impervious sometimes inaccessible to the semen owing to obliquity retroversion or prolapsion of the uterus. Cases have occurred in which impregnation has taken place under all these circumstances. The above clearly demonstrates that conception occurs though the semen be merely deposited in the vagina and warrants the conclusion that as a natural event it never penetrates into the uterus. Never=

-theless to remove all shadow of scepticism I will introduce what further light reason or experiments can throw on the subject. Experiments have been made by De Graef, Haller Leuenhoeck Haighton and others on different animals. The cow the ass the ewe the bitch the rabbit the doe have all been inspected immediately after connection with the male and never except in one instance could semen be discovered in the womb. Haller says that he found it in forty minutes after coition in the uterus of a sheep. This is a solitary case and deserves little attention when we consider that it was advanced in support of a favourite hypothesis. It is true that a story is floating about which reports that Mr. Hunter found semen in the uterus of a bitch but this is vague and not much insisted upon by the most strenuous advocates for the doctrine. In aid of Haller however it is urged that Morgagni detected semen in the uterus and Ruysch in the Fallopian tubes of the human species. Without impeaching the veracity of these celebrated men we must frankly say that their remarks stand in need of confirmation and we may strongly conjecture that what they mistook for semen was merely the mucus of the part. But even supposing their discoveries to be real what do they amount to contrasted with the vast body of opposing facts? They deserve not the slightest attention and weigh but as dust in the balance. It appears then that the semen does not enter the uterus, and of course does

not penetrate the Fallopian tubes. The latter position is merged in the former. But to silence all cavil I will take a cursory view of the considerations which would induce me to believe such an event impossible even though the semen should be applied to the mouth of the tube. That the Fallopian tube was not made for this purpose appears evident from its formation. Commencing in the uterus by an aperture not larger than a bristle it gradually enlarges and terminates in a fimbriated extremity. Now were it destined to convey from and not to the uterus the reverse would have been its construction. It is known that it carries the ovum from the ovary. By asserting that it also serves the former purpose we invest it with a twofold action of which there is no analogous example in the whole system. The inverted peristaltic motion of the intestines is the nearest though this is not a perfect parallel for it is a preternatural action and cannot be brought forward in illustration of one which is natural. But it is needless to protract the discussion as the point has been completely decided by the observations and experiments of Haighton. He inspected several animals from one to nine hours after coition and found that the Fallopian tube was in its natural condition with its fimbriated extremities hanging down loosely in the pelvis. He also found that the tube never takes the ac=

=tion by which it is enabled to embrace the ovary until the vesicle is matured and ready to discharge its contents He further proved by several experiments that even if the Fallopian tube previous to coition were so cut as to have its canal rendered impassible the vesicles afterwards shewed evident signs that the ovary had become fecundated. Convin=
=ced that the hypothesis in its primitive state was untena=
=ble its advocates were forced to resort to the supposition of an Aura Seminalis which penetrating through the uterus and Fallopian tube reached and penetrated the ovary. But here they were opposed by such a body of facts that they were driven back to the former theory. It might possibly happen that the Aura would penetrate where the semen would not but how are those cases to be got over in which the passage of the tube was rendered completely impervious and in which nevertheless concep=
=tion took place? Nor is this the only difficulty. I am not however disposed to enter into detail. It should first be shewn that the Aura has the power of fecundating. The experiments of Spallanzani and John Hunter prove the contrary. Distrusting the original hypothesis another class of Physiologists have resorted to general circulation as the means by which the semen reaches the ovaries. There are two branches to this latter doctrine but the difference is too small to deserve attention. No one has shewn that the semen in the blood retains its power nor if it does that it is particularly determined to the ovaries. Is it

credible that so small a quantity diffused through the whole mass of blood would be productive of such effects as result from a fruitful coition. Much stress is laid upon the experiments of Spallanzani who having dif=fused a small quantity of semen in a large quanti=ty of water fecundated with a few drops of this li=quor a great number of the ova of frogs. Were it as he states it would be a conclusive fact but subse=quent experiments have proved that semen is not sol=uble in water. Any one may convince himself of this by examining the ponds where he will find the ova of the female frogs and also the semen of the male floating on the surface. There are some species of fish which eject the semen and the ova in the wa=ter leaving them to be brought together by the accidents of wind and tide. After what I have said it cannot be doubted but that the results which Spallanzani obtained were owing to his having entangled with the point of his brush some of the semen which was floating on the surface of the water. But what analogy is there between solution and the combined operation of digestion and assimulation? To believe that semen would retain its powers after having en=tered the circulation is a stretch of credulity which is opposite to the dictates of reason and the lights of analogy. But this point is now beyond dispute. Ex=periments have decided that every article is entirely

changed in its nature before it can enter the circula=
=tion. We should be warranted in this conclusion were
it drawn only from the fact that the mildest fluids
such as milk mucilage &c. when injected into the
bloodvessels are productive of the most mischievous
consequences. The advocates for this hypothesis are not
aware of the ridiculous conclusions to which it would
inevitably lead. It follows from it that generation might
be carried on by inoculation and that by inserting se=
=men into the skin of a female we might raise a flock
of children as easily as we can raise a venereal bubo or
a crop of variolous pustules. Harvey indeed mentioned
that generation is effected somewhat in this way; he
believed that it was by a kind of contagion that the semen
acted so as to fecundate the ovary. Not less absurd was
the opinion of another Physiologist that the semen
passed from the toes of the male frog, through the axilla
of the female into the organs of generation. Nor that of a
third that in the coition of sparrows the female receives
in her mouth the seminal liquor of the male. We
need not be surprised that such preposterous doctrines
were adopted in the infancy of Science when we con=
=sider that hardly a day passes in which some vagary
equally absurd does not receive its supporters. Even Lin=
=naeus advanced the vulgar opinion that the female
of certain fish follows the male and swallows the
semen he discharges. Such hypotheses might raise

a smile if they were lireless or if they were confined in their effects to those with whom they originate, but they are far from being so. It is by them that learning is brought into disrepute and our science is exposed to the sarcasm of the witty and the contempt of the wise.

Lecture 2nd.

It may be said after all [illegible], however, that the veins do not [illegible] that they [illegible] set of absorbents [illegible] the [illegible] for the [illegible] of the animal [illegible] from the [illegible] to the [illegible]. If [illegible] can be admitted, it is not [illegible] the absorbent [illegible] probable [illegible] of their existence afforded. There, however, have been [illegible] many reasons to believe that they do not exist. The absorbents of the [illegible] as large and have been demonstrated as clearly as those of the other parts. There are two sets, one of which may be traced into the [illegible], the other into the [illegible] glands, while not a single one has been [illegible] in the [illegible] of the [illegible].

Nor would the [illegible] of absorbents be a sufficient confirmation of the hypothesis to which it [illegible]. It must be proved that they do not, like the veins, [illegible] the power of [illegible] that the [illegible] taken up [illegible] be so altered in its progress as to [illegible] by experiments. [illegible] of this [illegible] is to prevent [illegible] from entering the circulation [illegible] changes, and they are generally [illegible] to this and [illegible] however, they [illegible], the first ought to be

Lecture 2nd.

A very ingenious speculator of our own country, convin=ced that the semen does not enter the uterus, advanced a con=jecture that there were a set of absorbents running between the vagina and ovaries, destined for the conveyance of the seminal liquor from the former to the latter. Before such a deduction can be admitted, it must be demonstrated that absorbent vessels run in this direction, or some probable ev=idence of their existence afforded. None however have been advanced; on the contrary, we have every reason to believe that they do not exist. The absorbents of the vagi=na are as large, and have been demonstrated as clearly as those of the other parts. There are two sets, one of which may be traced into the sacral, the other into the In=guinal glands; while not a single one has been perceived running in the direction of the ovaries.

Nor would the existence of absorbents be a sufficient confirmation of the hypothesis to which I have alluded. It must be proved that they do not like the others, possess the power of digesting what they absorb, or we may infer that the seminal liquor, even if it were taken up, would be so altered in its progress, as to be rendered too manifest by experiment to be denied. One of their provinces is to prevent noxious articles from entering the circulation un=changed, and they are generally competent to this end. When however, they are not so, the first conglobate

gland arrests the further progress of the offending sub=
=stance and taking on inflammation expels it from the
system. These glands may be considered as centinels sta=
=tioned to preserve the body from being injured by the en=
=trance of any thing inimical to health. As yet there=
=fore the hypothesis must be considered as wholly gratu=
=itous built upon premises which cannot be proved and
on a course of reasoning which has been condemned by
Bacon Newton and all the disciples of that school of phi=
=losophy. "Give me a spot" cried Archimedes in the enthu=
=siasm of his genius "on which to erect my machines
and I will move the globe." Equally may the theorist
exclaim "Grant me my premises, and there is not one
of the arcana of nature that I will not develope." No=
=thing is more easy than to erect hypotheses; they a=
=rise from fertile imaginations like exhalations from a
pool, but these remember are deleterious to health; so
are false theories the bane of truth and the curse of
medicine. It results from what has been said that
the seminal liquor is not applied to the ovaries either
by means of the Fallopian tube the general circula=
=tion the Aura Seminalis or by a particular set of
absorbents designed for the purpose, but by a law of the
animal system called Sympathy or consent of parts.
Be not startled at this assertion. When the theory is
developed you will confess that it has stronger claims on
your attention than any one hitherto advanced. To

Mr. Haighton an experimental philosopher distinguished no less by vigour and sobriety of intellect than by vivacity of genius we are indebted for this beautiful specimen of inductive philosophy. It is regularly adduced from well established facts and comports with all the phenomena of generation with the changes which are produced by the uterus with analogy and with the laws of the animal economy. It has not indeed escaped opposition. No medical theory has been so perfectly constructed as utterly to defy an attack. The penetration of adversaries will always find or pretend to find some weak point some imperfection in the structure which they are always ready to publish to the world. It is to be regretted that Mr. Haighton did not defend his theory. Contented with refuting others he has thrown his own naked and destitute on the world to rise or fall according to its own intrinsic worth. As however the author has neglected the task it becomes my duty as one who has espoused the doctrine to say a few words in its defence. But before doing this let us say something of the nature of sympathy. Nothing is more certain than that in consequence of a sympathetic connexion between the several parts of our frame an impression may be communicated from one part to another or over the whole system. By some writers Sympathy has been divided into several kinds, as the contiguous or continuous remote and direct &c. distinctions however which are not necessary to our purpose. There are certain organs more

eminently endowed with this principle and with which the system more powerfully sympathises than others. Such are the Brain the Stomach and the Uterus. So great indeed is the influence of the last mentioned organ over the frame that an eminent Physiologist of antiquity considered it as a distinct animal controling the operations of the system and giving to woman her peculiarities. At a comparatively modern period Van Helmont mentioned that all the diseases peculiar to the female sex are owing to this organ and even went so far as to affirm that "propter solum uterum mulier est quod est." Two objections have been urged against this theory of Haighton. The first is that it is contradicted by analogy. The experiments of Spallanzani it is said shew that the ova are fecundated by the seminal liquor as they are discharged from the female. Every one will admit the fact that the business of fecundation in frogs may be artificially accomplished. But we cannot be too slow in adopting analogical reasoning in defence of a favourite theory. Analogy serves better as illustration than argument and should ever be appealed to with the most cautious circumspection. In the present instance it is very remote and the cases are entirely different. In the animal above alluded to impregnation takes place out of the body and nature

could adopt no other course than that which she pursues. It is again urged that impregnation is effected in the same manner in vegetables. It may be so though strong doubts are entertained by many and by Logan and others it was utterly denied. As regards some plants it is universally admitted that the organs of generation are so constructed as to preclude the possibility of the pollen reaching the ovary. In these conception must be accomplished by something like sympathy. The credit of arranging the sexual system is due to Linnaeus, but is was Logan of this city who first suggested the idea and whose experiments on the generative process in corn were communicated to the Physiological: Society in London before Linnaeus wrote on the subject. It appears to me that analogical arguments drawn from birds are deserving of much more credit than those from frogs or plants. Here the process of generation is carried on in the body and the structure of the organs is not very unlike that of the human species. In birds the ovaries are situated high up on the spine and they have the infundibulum which may be compared to the Fallopian tube. Their uterus is long and convoluted like an intestine. In copulation the male being without a penis or any power of projecting the semen merely deposits it within the vulva of the female. Notwithstanding this the ovum becomes fecundated at once. The fact originally noticed by Harvey has lately been confirmed by ample experiments. Can it be credited that so extensive an impreg=

=nation is effected by the contact of the semen? Let it be remembered how long is the uterus how high up on the spine are the ovaries and how difficult of access. The force of this being admitted it is still objected to the doctrine that there are many phenomena of generation for which it does not account and that it is difficult to imagine how conception can take place from sym=
=pathy. How for instance can we explain by it the fecundation of the ova the resemblance of the child to the parent the transmission of hereditary diseases the production of a mule from the union of two animals of a different species &c.? In every view of the subject we are involved in obscurity and difficulty but they are not increased by the doctrine we are advocating. Does the ancient hypothesis of the direct application of the semen unravel the perplexity? It does not af=
=ford us a single ray of light. Whatever then are the effects of this theory they will apply with equal force to every other. There are mysteries in conception which elude human research and most probably will never be revealed. It is obvious to me that the principal dif=
=ficulty which opposes Mr. Haighton's theory is the remaining partiality to the humoral pathology much of which is retained even in the present day. By the humoral pathology it was held that all articles are conveyed by the circulation to the part on which an impression is made. Whether noxious or medicinal

they are supposed equally to pursue this rout. No other mode of action was thought to be conceivable than by di=rect touch. Let us now see whether we can render our the=ory acceptable even to the humoral pathologists themselves. Whenever any agent medicinal or poisonous is applied to a susceptible part internal or external an action is excited which is extended more or less according to the diffusible nature of the article or the degree of affection which exists between the part affected and the body generally. The action thus ex=cited is the same in the same system by which I mean in parts of a similar structure and destined for a partic=ular purpose. If it runs into other systems the action is dis=turbed and broken by the different organization of the part. To illustrate my meaning I will state a case. By inserting some variolous matter under the skin we excite a local irri=~~tation~~ which in a few days is diffused producing fever and pustules are thrown out which resemble each other be=cause they will occur in the same system or order of parts. In this way every morbid action is communicated when the dis=ease commences at a point. The matter is not infinitely dis=solved in the mass of blood but in the place where it is applied it excites an action which is propagated by sym=pathy all over the system. Whatever operates on the living system is obedient to the same laws. There is one spot in which the action commences from which as a focus it ra=diates to every part around it. By adopting these views, we have a satisfactory method of explaining the operations

of the seminal liquor. The uterus Fallopian tubes ovaries and vagina constitute a system between the parts of which there is a close sympathetic connexion. Let us now trace the phenomena as they exhibit themselves after coition. Deposited in the vagina the semen begins its stimulant operation this is quickly communicated to the uterus and finally to the ovaria. In consequence one or more of the vesicles enlarges becomes red and opaque projects and at length bursting discharges its contents. In the mean time the Fallopian tube has been undergoing a change which renders it able to rise embrace the ovary and receive the contents of the vesicle as it bursts. This change consists in a gradual turgescence of its vessels which renders it stiffer and by degrees raises it from the cavity of the pelvis. After it has performed its office by conveying the ovum to the uterus it returns again to its former state. While these operations are going on in the appendages others equally important are brought about in the uterus itself. That organ is engaged in fabricating the membrana decidua to afford a receptacle to the ovum to guard against the escape of which the os tincæ is completely closed by a thick viscid mucus secreted for that purpose. Nor does the operation stop here. It is necessary to provide nourishment for the child after it has escaped from the womb and for this purpose the breasts are gradually

enlarged and prepared for the secretion of milk. Every part of the above process has been so well ascertained by experiment and observation, as not to admit of doubt or dispute. Tracing then these actions through the ovaries Fallopian tube uterus and breasts of the mother we shall find that they are links of a great chain and that generation arises from that law of the animal economy called association or sympathy. That a portion at least of them are of this nature cannot be de=nied. Every one must acknowledge that it is from sympa=thy with the uterus that the breasts swell. But if parts so remotely situated can be so affected why should not the several parts of the uterine system. Consider well what has been said and you will not withold your assent to a theory legitimately deduced from facts well ascertained and bright=ened by the lights of reason and analogy.

Lecture 3rd.

I next proceed to an examination of what have been called the theories of Generation. You are apprised that on no subject has human genius been more actively employed, and more ineffectually so, than in the creation of hypo=theses to explain this dark and intricate process. It is hardly to be credited though it is indisputably true, that so long ago as one hundred years, there existed no less than 262 theories relative to generation. It is needless to re=mind you that since that period the fertility of inven=tion has by no means diminished, and that there has been a proportionate multiplication of doctrines. Do not think I mean to conduct you through this entangled wilderness. The conjectures of the ancients being founded on the presumption of a mixture in the uterus, an idea now entirely abandoned, and being of a metaphysical nature, I shall very cursorily examine or altogether omit it. An acquaintance with exploded doctrines may be knowledge, but in the words of the elegant Burke, it is barren knowledge; a species of intelligence of no practical advantage. These theories have long since been shewn to be the product of medical philosophy when Science was in her infancy. Numerous however as they are, they may all be arranged under two heads or di=visions. (See my edition of Richerand, Page 534 and read from the beginning of the note to a mark about

the middle of the next page.) It is humiliating to the lo=
=vers of truth and science to dwell on the false reports made by those whose minds where perverted by the ardour of their pursuits or whose veracity was warped by their ambition to support peculiar notions. No sooner had Leuenhoeck made the discovery of spermatic worms than he and his followers pushing their investigation still farther discovered or pre=
=tended to discover their form structure movements and habitudes. He affects to have seen a million of these animal=
=culæ in a drop of semen not larger than a grain of sand. Dr. Darwin facetiously remarks of these animalculæ that they must have been even more minute than the devils which are said by one of the monkish legends to have tempted St. Anthony 20,000 of whom danced a Spanish fandango on the point of a needle. Conformably to the observations of Leuenhoeck the spermatic worms exist in the semen of all animals in that of men birds quad=
=rupeds fishes and insects. They are long slender and ap=
=parently without any extremities. They have considerable motion not only in their tails but also in their bodies so that they sometimes entirely change their position. All these points were corroborated by his pupils some of when went even further. One of them asserts that he saw even with his own eyes a spermatic worm quit its covering and become a perfect human being; and ano=
=ther declares with equal assurance that he had observed one of them in the semen sitting exactly in the same

posture as the foetus in utero. Even the enlightened Boerhaave so far bent under the prevailing infatuation as to tarnish if any thing can tarnish the brilliancy of his reputation. He says that he saw in the semen of a ram a flock of animalculæ pursuing each other just like a flock of sheep rushing into the pen. When however I state that among the advocates for this doctrine were ranked the most intelligent philosophers of the age you will readily agree with me that it must have been plausibly made out and powerfully supported. Those who wish to enquire more particularly into the subject may consult the third volume of Buffon's Natural History. In the same work may also be found a satisfactory refutation of the doctrine. This was long since repudiated but has lately been revived by Darwin in a different form. After rejecting the commonly received opinion that the semen acted only as a stimulant he maintained that the animalculæ which he called living filaments were secreted from the blood with the seminal liquor and served as the germ from which the future animal was to spring. But there is no such filament in the semen of the male. In that respect it is not different from the other fluids of our body. If it contains animalculæ these have nothing to do with the process of generation but are of the same class with those which are found to pervade all nature and des=

=tined to form one of the links in the great chain of ani=
=mal existence. In constructing this theory he has evidently
had his mind fixed on some inferior animals and plants
which are propagated by branches growing from themselves
On this deceptive analogy he seems to have founded his
theory. Had he confined it to plants and the animals a=
=bove alluded to he would have done right. He should have
considered that the modes of generation are greatly diversi=
=fied and have attended to what he well knew that nature
seems to take delight in varying the process in different
animals. We have now completed a view of the theories
which suppose that the germ pre-existed in the male.
They are all overturned by the fact that the primary step
of the generative process is in the ovary; for how could one of
the spermatic worms reach this organ through the long
vent of the vagina uterus and Fallopian tube? There was
however one of the advocates for this doctrine who pretend=
=ed to have discovered on of the animalculae in the
very act of performing the journey. In the progress of my
lecture I remarked that the ovular doctrine was refuted
by Leuenhoeck. After a while however it again revived
under the auspices of Haller. [See page 535 of my
edition of Richerand.] They rest their theory upon the
following propositions, deduced they say, from experiments.
They say that the capsule of the vesicle contains a trans=
=parent homogeneous fluid, which never takes an action
till it has been excited by the fecundating influence

of the male semen. How are we to determine between the contradictory statements? First let us set aside all au=thority and all our prejudices; next let us examine the subject and see what doctrine is most consistent with established facts. I must confess my own utter incredulity with respect to pre-existing germs. Each branch of theory is equally incorrect. Bear in mind that they both suppose the germ to be a miniature of the parent and differing only in being on a smal=ler scale. Such however is not the primitive appear=ance of the embryo. In its early state the foetus is a "Rudis indigestaque moles." From this rude im=perfect condition in which hardly any signs of or=ganization appear it is developed by a slow and grad=ual progress. The primary aspect of the human em=bryo bears no resemblance to the figure it is destined to adopt. The same occurs in other animals. In what respect is the tadpole like a frog, the chrysalis like a butterfly the pellitory shoot to the perfect plant? Do we in these see the exact miniature of the pa=rent's form? But this is not my only objection. It is impossible to reconcile with the doctrine of pre-ex=isting germs the recovery of lost parts. Yet the polypus has this property, and it exists, though in a less degree, in the more perfect animals. No one will say that Nature foreseeing such an accident has provided another germ yet this is the only way of esca=

-ping the difficulty. Nor is the production of hybridous animals more intelligible by the doctrine on which we are animadverting. If the germ preexists in the horse or ass, how does the mule happen to be an exact compound between the two? How in our species are we to account for the existence of mulattoes? Supposing the germ to have been in the father how should the child resemble the mother, and vice versa? How are we to explain the inheritance of certain diseases, as gout, scrofula, rickets &c? Temperament disposition and peculiarity of structure may all be inherited from either parent. Sometimes the child both in body and mind is almost an exact compound of its parents. To get out of these difficulties some speculators have attributed a plastic power to the semen by which it moulds the embryo into certain forms. But if this faculty be given to the seminal liquor what becomes of the perfect animalcula? The influence of the mind has also been called in to assist in explaining the phenomena. To this we may reply that whatever power the human mind may possess over conception very little influence of this kind can be attributed to the lower class of animals and to plants. The loves of plants make a conspicuous figure in poetry but the vigour of their imaginations remains yet to be proved. So much for the doctrine of pre-existing germs. It is highly plausible, and were it just, would not fail to obtain many admirers and followers.

Lecture 4th.

I have now completed what I had to say upon the doctrine of the evolution of the germ, and I think I have shewn that however plausible, it possesses no solid claim to our attention. The next theory that comes under our notice, is that of Epigenensis. Discarding the noti=on of the pre-existence of the germ, it presumes these prepared, but at the same time unorganized rudiments of the foetus, first begin to be gradually organized when they arrive at their place of destination, at a due time and under the necessary circumstances. In other words, denying the pre-existence of germs in either parent, the doctrine of Epigenensis supposes that the fluid contain=ed in the ovarian vesicle, is the rude elementary matter which after impregnation becomes organized in the embryo by the energies of the semen masculinum. The primary traces of this doctrine are to be found in the writings of Aristotle. The prevailing opinion on the sub=ject of generation in the time of this philosopher, was that each sex furnished semen, and that the embryo resulted from a mixture of these two fluids in the uterus. After confuting the popular opinion that females have semen, he asserted that they contribute nothing to conception but the menstrual blood; that the rudiments of the embryo are derived from the menses, and are vivified and put together by the plastic power which he imputed to the

semen. Thus, according to his doctrine, the seminal fluid is the sculptor, the menstrual blood the marble, and the foetus the figure. With various modifications this doctrine has been handed down to us. It would be impossible to point out all the shapes it has assumed at various times. Of late its most able and determined supporter is Blumenbach. I will read you his own account of his doctrine (vide Blumenbach's Physiology.) In other words we might thus enumerate it. The male semen, and the liquor which is secreted by the ova=ria of the female, are mingled in the uterus, and formed into the foetus by the energy of vitality, or as he denominates it "Nisus formativus." This hypothesis in the outline appears not very far from truth. But in filling up he has committed errors of so gross a nature as to impeach his reputation as an accurate Physiologist. It is not true that a commixture of the seminal liquor with that of the ovaria takes place in the uterus. Nor is it less certain that the albuminous liquor of the female does not reach that organ until a considerable time after fecundation; perhaps 20, 21, or 22 days. Tired of criticising this subject, I shall not delay to point out all the errors of the doctrine which come under our no=tice. My own conviction, which as you may readily per=ceive is the result of much enquiry and reflection, may be stated in a few words. I believe that the ovary is a gland which secretes the rudiments of the embryo. These consist at first of merely a pellucid fluid contained in the vesicles of the ovary. But in consequence of the semen masculinum

extended I believe by sympathy life organization and figure are communicated to the rude materials. That the ovaria perform this office admits almost of demon=stration. Experiments have clearly shewn that at the age of puberty this organ takes on a secretory action which undergoes a change at every fruitful coition. Thus altered it is introduced into the uterus where by subsequent elaboration it is converted into the foetus. Do we not see the egg perfected by the ovary in all the parts neces=sary to conception and requiring only the fecundating influence of the semen masculinum to render it prolif=ic. By analogy then as well as by experiment we are as=sured of this doctrine. Nor is there less evidence to prove that the contents of the vesicles are moulded into shape by the agency of the seminal fluid. It cannot be doubted but that this to a certain extent possesses a plastic energy. The fact indeed is manifested by the revolution which the system undergoes when it first receives the impression of the semen and will not be doubted by any one who is not exceeding unobser=vant or irrationally sceptical.

At the period of puberty several very striking alterations take place. Hair grows on the pubis, the beard begins to appear, the voice for the first time becomes harsh and dissonant; the countenance con=tracts a new expression, the form generally improves, and a striking change is observable in the disposi:

=tion of the heart and the faculties of the mind. The powers of the semen may be still more strikingly illustrated by comparing castrated animals with those which preserve their functions entire. But why occupy our time with the relation of instances which must be observable to you all. With regard to the nature of the seminal agency we are not well informed. Semen is a peculiar fluid producing effects "sui generis". The same language we are compelled to hold respecting other substances. It is as difficult to ex=plain the action of the variolous matter in producing small-pox or of mercury in curing syphilis. I have nothing more to say upon the subject of generation. What I have said has been advanced with diffidence in consequence of the peculiar obscurity and difficulty of the subject. The whole ground is now before you and you must judge for yourselves. But whatever opinion you adopt it will undoubtedly be some modification of the doctrine of Epigenensis. Compared with the other it is decidedly superior. It comports better with the phenomena as they have been detailed and af=fords a solution of circumstances which are irreconcil=sable with the pre-existence of the germ either in the ovaria of the female or the semen of the male. In the prosecution of our enquiries the demonstration or rather the description of the ovum comes next in order. By this we mean the sac or bag which is found in the gravid uterus inclosing the foetus and its appen=dages called the secundines. But before going further, it

will be proper to discuss a point or two of importance. As yet it is doubtful in what form the rudiments or primor=dia of the foetus are transmitted into the uterine cavity. By De Graef and the supporters of the ovular doctrine it is maintained that they are a true and perfect ovum "ab initio," or from the time they escape the ovaria. But Farialli and many of his cotemporaries hold a contrary sentiment. It is denied by Haller that they have any vesicular structure until they arrive in the cavity of the uterus and have remained there several days. The same account is given by Haighton. As the researches of these two last were undertaken with opinions contrary to the result they deserve the more attentive consideration. It is due however to candour to state that there were not wanting many who were eminent physiologists to sup=port the opinion of De Graef. Among these is the celebra=ted Cruikshank whose observations were no doubt made with great care. When circumstances so contradictory occur it is often very hard to decide between them. In weighing the evidence of both parties I confess it seems to me most probable that the discharge from the ova=ries into the uterus is a mere fluid without any invest=ing membrane. The Fallopian tube is so small that it would seem almost impossible that any thing but a flu=id could pervade it. May we not account for the conflict=ing opinions on this point by supposing that the thick al=buminous fluid which the ovaries pour into the Fallopian

tube receives a globular form in its descent and thus deceives those who mistake it for real ovum. From this circumstance independent of the experiments of Haller and Haighton it would seem probable that their opinion is the true one. Equally disputed is it at what period after fruitful coition the rudiments enter into the uterine cavity. In the brute creation this point is easily ascertained. But so variable are the laws which govern the different species of animals in this respect that it would be impossible from what is observed in the lower species to draw any certain inferences as to the human. As examples of this difference I will first state that in the uterus of the rabbit whose period of gestation is thirty days the primordea of the foetus is discernible on the 5th and little more than that time elapses before the ovum is found in the deer whose pregnancy continues nine months while the ewe which produces 5 months after fruitful coition does not contain the ovum in the uterus before the eighteenth day. In our own species from the best testimony I can collect I conclude that three weeks pass before the rudiments of the child are to be found in the uterine cavity. The conclusion is the result of modern investigation. Anciently it was believed that the liquor of the ovaria descended immediately at the time of coition. Buffon has gone so far as to give a picture of the foetus at one week old delineating even its features. But this is entirely the creation of his own fancy. The primordea when first observed appear like a

cloudy speck contained within a duplicate bladder and suspended in a pellucid fluid. Not long after this period the embryo becomes more organized but is still very im=perfect. Soemerring says that between three and four weeks after conception the ovum becomes invested with two membranes the Corion and Amnion which are tin=ged with a fluid and measure in diameter about five lines. When washed with spirits of wine a small speck or line is seen suspended by a chord and the superior and inferior extremities appear like the germ of a plant. I remarked on this subject that from the best evidence (although this point could not be demonstrated) that the time which elapses from the time of fruitful coition to that at which the primordia are first discovered in the uterine cavity is about three weeks. The first thing perceived at the end of the third week is a small speck enveloped in mucilage. This speck after a while assumes an ovular appearance and increasing exhibits at length the foetus itself about the size of an ant suspended by a chord as fine as an hair. At the expiration of the sixth week after the most common time of abortion in women we find the child about the size of a common bee and weighing a scruple. At this time very little appearance of the human form exists. The foetus seems to consist of two parts joined to=gether the one resembling the head the other the trunk of the body. The features begin to be somewhat though

indistinctly marked. The eyes are prominent a line is visible representing the mouth small protuberances appear which are to form the ears and nose and the upper and lower extre=mities begin to pullulate. After eight weeks the child grows with more rapidity and its parts are very speedily developed. Ninety nine out of an hundred women abort about the end of the third month. The fœtus is at this time about three in=ches in length. From the 5th month its increase is still more rapid. At the 6th month it is about nine inches in length and weighs 4 lbs. At the 7th it has increased to 12 inches and weighs 6 or 7 lbs. By the 8th, it measures 15 or 16 inches and has a proportionate increase in weight. At the expiration of the 9th month which in our species is the general time of delivery the infant weighs from seven to twelve pounds and is from 15 to 22 inches long. From my own observations I can bear testimony to the correctness of this representation. I once had an opportunity of exam=ining the contents of an abortion which took place on the 20th day after menstruation. The ovum was about the size of a nutmeg the coats were transparent and disten=ded with the liquor amnii which was as clear as water and the embryo resembling a large ant was visible floating on the liquor and suspended by a cord half an inch in length and as delicate as the finest thread. The little foetus was divided into two nearly equal portions by a fissure that surrounded it. It is much to be regretted that the products of early abortions have not been oftener

subjected to examination. The light of comparative anatomy may in some degree compensate for the want of more accu=
=rate knowledge. To this then we must appeal. De Graef declares that in the uterus of a rabbit he was unable to discover any thing like the rudiments of the foetus before the eighth day and that it appeared as a small cloudy speck situated in the centre of the ovum. On the 9th day it had become more distinctly visible and went on increasing until the 12th when he discovered signs of the head and extremities and observed two red points in the thorax. On the fourteenth the head was formed the eyes were prominent the mouth open the ears dis=
tinguishable and the trunk elongated; the puncta sanguinea had increased and now evidently appear=
=ed to be the rudiments of the ventricles of the heart, and on each side was seen a white spot representing the abdomen. He found the germs of the stomach, in=
=testines, liver, spleen, and other viscera. After the 14th day the parts rapidly advanced until the 29th when the whole was completed and the rabbit delivered. All the above circumstances are confirmed by Mr. Haigh=
=ton who with his usual accuracy repeated the exper=
=iments. He stated that he never could discover any thing in the uterus of a rabbit earlier than the 6th day and that then there was only a cloudy ap=
=pearance. On the tenth day an opaque spot was to be seen, which progressively increasing in bulk, at length

was developed and at the usual time perfectly formed. It is surprising that when the term of utero-gestation is limited to so short a period as one month a third of the time should be employed in the production of entity. It seems to require almost as much time to form the nucleus of the foetus as to go on and complete the work. Experiments on eggs were attended with the same results. Harvey informs us that until the 5th day there is no appearance of the embryo and that even then it is hardly discernible being a mere line which he compared to the keel of a ship or worm having a small body repre=sentinq the head attached to one end. On the sixth day the head becomes prominent the legs begin to appear and the germs of the abdominal viscera are discernible. It is said by him that the heart is first seen and then the lungs. From this time the chick gradually increases un=til the 20th day when the term of incubation is com=pleted. These experiments have been confirmed by others conducted under my particular notice by a graduate of this university. To the question whether the blood exists before the heart and arteries Harvey answers universally in the affirmative. But his experiments do not afford suffici=ent ground for so positive a conclusion. We shall it is probable never be able to satisfy ourselves on this subject. The subtlety of the subject is so great as to elude our resear=ches nor can we gain any thing from a priori reasoning. It may indeed be urged in support of Harvey's opinions that every part that enters into the constitution of the body

is derived from and supported by the blood. But it may be rejoined that the blood is the production of an elaborate process in which the heart and arteries are concerned and cannot therefore have a priority of existence. Happily however such questions are more curious than important in a practical point of view The foetus in six weeks is not larger than a common fly in seven almost the size of a bean in eight nearly double that size and the features observable. It occupies that position in the uterus which takes the least room. The trunk is bent forward the chin is pressed down on the breast the feet are drawn up the thighs applied to the abdomen and the arms across each other. Till lately it was thought that the foetus at first sits upon its posteriors in the uterus and about the 4th month by performing a kind of somerset assumes the posture which it appears to have at delivery viz. with the head downward. No question ever engaged more closely the attention of Physiologists, nor excited more warmth of discussion than in what manner the foetus performed that motion. At length a person more cautious than the rest settled the dispute by proving that the position is not altered but remains the same during the whole period of gestation.

Lecture 5th.

[illegible] the pregnant uterus which it [illegible] of [illegible]
[illegible] of the [illegible] perfectly [illegible]
that the [illegible] the [illegible]
of Physiology. [illegible]
necessary to say a few words on the [illegible] and [illegible] appendages
[illegible] It has already been [illegible] that you [illegible]
understand by the [illegible] in the
the cavity of the gravid uterus, and [illegible] the [illegible]
[illegible] appendages. It consists of [illegible] membranes, one of which [illegible]
[illegible] to the [illegible], and the other [illegible] the [illegible] of the [illegible]
[illegible] The first one is called the [illegible] and [illegible]
in the latter stages of pregnancy, are in close contact with [illegible]
that but it [illegible] slightly separated by the [illegible]
of a [illegible]
[illegible]
[illegible] the [illegible]
[illegible] portions of the [illegible], which [illegible] the [illegible]
[illegible] of the [illegible]
but this [illegible]
place in the [illegible]
[illegible] of which is the [illegible] of this [illegible]
[illegible] it was stated that [illegible]
of the [illegible] which are [illegible] with each other, [illegible] the [illegible]
[illegible] the [illegible] Dr. Hunter [illegible] to the
coagulation of the blood [illegible] Dr. Hunter says that

[illegible] from [illegible] by the blow. But it may be agreed that the blow is the production of an elaborate process of which the first [illegible] are [illegible] and cannot therefore have a penalty of [illegible]istence. Happily however such questions are more curious than important in a practical point of view. The foetus in six weeks is not larger than a common fly or [illegible] about the size of a bean in eight weeks by which time [illegible] the features observable. It appears that [illegible] in the [illegible] whichever [illegible] the first [illegible]. The mouth is first formed the [illegible] is [illegible] on the breast the [illegible] up the thighs applied to the abdomen and the [illegible] each other. Till lately it was thought that the foetus at first sits upright [illegible] in the uterus and that the [illegible] by performing a [illegible] of [illegible] [illegible] which it appears to have [illegible] [illegible] [illegible] [illegible] of Physiologists [illegible] [illegible] than [illegible] [illegible] [illegible] [illegible] [illegible] [illegible] [illegible]

Lecture 5th.

In the present lecture I shall enter into the subject of the nourishment of the fœtus in utero. It is perfectly known to all that this is one of the most intricate and least understood parts of Physiology. As preliminary, however to the main point, it is necessary to say a few words on the ovum and its appendages the secundines. It has already been mentioned that you are to understand by the ovum a membranous sac or bag, found in the cavity of the gravid uterus, and containing the foetus and its appendages. It consists of three membranes, two of which are peculiar to the foetus, and the third the production of the uterus. The first two are called the Amnion and chorion. These in the latter stages of pregnancy, are in close contact with each other, but at first are slightly separated by the intervention of a mucilaginous matter. The amnion is the internal membrane and situated next the child, serves as a lining for the ovum. Next is the chorion, and on the outside of this is the reflected portion of the decidua, which forms the external coat of the ovum. Much difference of opinion has existed, but it is now well ascertained that when conception takes place in the ovaria, the uterus assumes a new action, the object of which is the fabrication of this membrane. By Haller it was stated that vessels sprout out from the surface of the uterus which are woven with each other, and thus produce the decidua. By Jno. Hunter it was attributed to the coagulation of the blood. His brother Dr. Hunter, says that

it is the result of an efflorescence of the uterus. The prevailing opinion is that it is the product of an action similar to that by which the membranes of inflammation are produced. Scarpa I think is the author who avers that he has made ex=periments which render this certain. I do not know these experiments but there certainly are some circumstances of resemblance between the two membranes. They have the same colour and texture each being pulpy vascular and tender. The membrane of coagulable lymph is formed by the process of inflammation. The uterus when employed in fabricating the decidua is in a state of high excitement. But here the resemblance ceases; the membrane of inflamma=tion exists only a short time in its original form being soon secreted into the cellular membrane of the body while the other exists for a considerable time and performs functions sui generis. We are not then warranted in attributing to an identity of action substances so different in their offices and powers. The uterus may be excited but not into a state of inflammation. There is no affinity between in=creased natural action and one which is the consequence of disease. We must therefore consider the decidua as a peculiar membrane the result of a specific operation of the uterus. Physiologists have been doubtful on this point. They are no less so in the attempt to explain the reflect=ed portion or that which forms the envelopement of the ovum. To me however there does not appear to be any great difficulty. The decidua which before the entrance of the

ovarian fluid gives a complete lining to the uterus is composed of two layers. That which lies near the uterus is perforated in two places where the Fallopian tubes enter and the other is entire having no opening in it. Now when the contents of the vesicle reach the mouth of the tube one of these things must happen. The ovarian fluid must either be arrested at this spot or it must lacerate the internal membrane. The former circumstance really happens and the ovum is at length covered by the protruding membrane. Hence the portion which envelopes the ovum is called the decidua reflexa, and that which lines the uterus decidua vera. The animal economy affords one portion in a striking manner analogous to this. I allude to the descent of the testicle into the scrotum in the foetal state. This gland lies on the spine posterior to the peritoneum in its descent along the back it pushes this membrane before it till at length it reaches the scrotum where the peritoneal coat is called Tunica vaginalis. Precisely in this manner does the ovum protrude the decidua before it forms for itself a reflected covering. The next point that must receive our attention is the formation of the placenta. In order to comprehend this it will be necessary to call to mind that the ovum is completely invested by the reflected portion of the decidua, between which and the chorion an intimate union takes place by means of inosculation of vessels. That such an union exists may be demonstrated by maceration. The decidua and chorion thus connected form a bed or matrix exceedingly soft and pulpy in its nature. In this bed the vessels of the umbilical cord enter

and ramify in all directions while on the other hands the vessels of the uterus do the same. This also may be demonstrated by maceration. After a while cellular membrane is formed which is interposed between the vessels of the umbilical cord and those of the uterus and the whole structure become paronchymatous having a striking resemblance to that of the lungs. The placenta is formed in all the more perfect animals but is much diversified in its structure In some the vessels terminate in the ovum without any intervention of the cellular membrane. In other quadrupeds as in the mare the uterus throws out small prominences which are received into corresponding depressions on the foetal portion. The depressions from their resemblance to cups are called Cotyloides and the projections are denominated papillæ. In the third class as the dog, cat, rabbit &c. each foetus has an independent placenta. In the human species it is different from the same organ in brute animals I mean in its being caducous, that is coming away with the foetus and secundines. In no other species with the exception perhaps of the monkey is the whole placenta shed. The foetal portion alone comes away. I shall add no more respecting it at present than that it serves as a connexion between the mother and child. The umbilical cord which ramifies in it is in general composed of two arteries and one vein in the human subject.

To complete our account of the ovum and its appendages I will say a few words relative to the liquor Amnii or that

collection of fluid which exists within the cavity of the amni=
=on. During the early stage of pregnancy it is pure and limpid but
afterwards becomes contaminated and sometimes from admix=
=ture with the meconium very dark putrid and offensive. There
have existed various opinions relative to its origin. It has been
supposed to be the perspiration urine saliva and even the mucus
from the nostrils of the child. But the liquor amnii cannot pro=
=ceed from the foetus because it exists in large quantities before
the organs which could have produced it are developed, and it
is found in cases where the ova has been blighted and the foetus
possesses no organization. By Haller it was thought to be a se=
=cretion of the uterus which transuded through the membranes
of the ovum. But independent of the great doubt whether there
is such a thing in the human system as transudation thro'
a membrane how can this opinion be reconciled with the
fact that the liquor amnii exists also in extra-uterine con=
=ceptions? It seems to me most probable that it is an exhala=
=tion from the arteries of the amnion. With regard to its u=
=ses a variety of sentiments have been entertained. It was
once supposed that this liquor served as nourishment for
the foetus, but there is no foundation for such a supposi=
=tion. Its uses are twofold. In the first place it protects the
foetus from its conception and gives it room to extend itself
by growth. Secondly, it promotes labour by gradually en=
=larging the os tincae which it does by being pushed
downwards and insinuating itself like a wedge and more=
=over keeps the uterus distended enabling it to act with more

force. We see this last advantage exemplified by a tedi=
=ous and difficult labour which results from a premature
rupture of the membranes.

I have only to add relative to the secundines that they
possess no apparent vascularity except the cord pla=
=centa and amnion and that no absorbents or nerves can
be traced in their composition. They have no fat either in
a diseased or healthy state. The membranes do not ex=
=hibit a fibrous appearance but seem like dense gluten
or coagulable lymph. Being designed to remain with=
=in the body but a short time their structure is suited
to the end which they are destined to fulfil. We now
proceed to give an account of the nourishment of the
foetus in utero. Numerous as are the speculations
on this subject they may all be referred to one or other
of these sources. First, that the child derives its sup=
=port from the liquor amnii. Second, that nourishment
is conveyed to it from the umbilical vessels. In another
place (the Eclectic Repertory) I have examined the
grounds upon which the first opinion rests and have
shewn that its only claim to notice is the respectable
names that are attached to it. Contented with referring
you to this book for a more complete account of the ob=
=jections against this doctrine I shall at present men=
=tion only some of the most prominent. 1st. The liquor
amnii cannot serve the purpose attributed to it because
it is not nutritious, being entirely devoid of those pro=

=perties which could render it a substance capable of supplying nutriment to the frame. Nay in the latter stage of gestation it often becomes feculent acrid and putrid. 2nd. Its quantity is not in inverse ratio to the size of the foetus and sometimes it is almost totally wanting. I had however a case where the membranes were ruptured a week or more before delivery and yet the child when born shewed no signs of emaciation. 3rd. The foetus has in some instances existed with the intestinal canal so closed that it would be impossible for any fluid to enter it. I have seen as many as thirty cases of this nature. 4th. Previous to the expiration of the third month the stomach and intestines are in a pulpy condition totally unable to perform any action by which elementary matter could be con=verted into chyle. It would seem that during the growth of the foetus no organic function should be performed. The evoluti=on of the different parts is the only one that is aimed at. All the organs with one or two exceptions remain inactive. The heart and bloodvessels are the only ones that act to any great extent. The brain is endowed with its peculiar energy. The stomach and intestines are without the power of digestion, the glands without secretion the muscles without motion the senses without sensation the absorbents without absorption. Whatever therefore may be the precise mode of nourishment the organic action of the foetus has little concern in the process. No other proof of this is want=ing than that the foetus continues to grow though destitute of one or more of these organs without which after birth life could not be retained. We have many cases on record which state

that the foetus attained its full size though destitute of some of the following parts, viz. The brain heart lungs and several of the abdominal viscera. The subsistence of the foetus is purely parasitical. Its food is prepared by the organs of the mother and is wholly destitute of excrementitious parts before it enters the child. On this account it is that there are no excretions in the foetus. Urine upon examination has not been detected in the bladder and what is called the meconium is not from food of which the nutritive matter has been extracted. I have completed the refutation of the first hypothesis and shall proceed to the second which is in itself so plausible that it challenges our most serious consideration.

Lecture 6th.

The theory concerning a circulation, may be traced to the remotest period of antiquity. It was taught in the school of [illegible] ancients, but with what accuracy of detail it is impossible to [illegible] [illegible] among [illegible] of the [illegible] of the blood, and subsequently [illegible] the prevailing opinion [illegible] and was long after it was generally believed that there existed a direct vascular communication between the foetus and the parent through the medium of the placenta. But the contrary at present is [illegible] by general [illegible] that there is no point in [illegible] more completely settled. This being the case, it is unnecessary to [illegible] by entering into a [illegible] of the [illegible] circumstances which render the opinion of a direct communication no longer tenable. [illegible] it seems [illegible] by all means, and for this purpose shall state some of the objections which may be urged against it. The allegation made [illegible] communication between the foetus and parent is disproved 1st by the total failure of all attempts to [illegible] the [illegible] 2ndly. By want of corresponding pulsation in the [illegible] blood [illegible] and maternal arteries. 3rdly. By the difference between the foetal and maternal blood. In these [illegible] my [illegible] a consideration of great weight namely that [illegible] contended for the [illegible] the mother and [illegible] the foetal economy, a [illegible] of what [illegible] takes place [illegible]

that the germ [illegible] though [illegible] of one of the following parts, viz. The brain, heart, lungs and several of the abdominal viscera. The substance of the foetus is [illegible]. Its food is prepared by the organs of the mother and is wholly [illegible] of [illegible] the child. On this account [illegible] in the foetus. Having [illegible] examined [illegible] and what [illegible] the [illegible] from [illegible] which the [illegible] has been [illegible]. I have completed the refutation of the first hypothesis and shall proceed to the second which is in itself so plausible that it challenges our most [illegible] consideration.

Lecture 6th.

The theory now under consideration, may be traced to the remotest periods of antiquity. It was taught in the school of the stoicks, but with what disciple of that school it originated, ~~is~~ is not known to me. Entombed during many centuries, it was again revived about the era of the discovery of the circulation of the blood, and subsequently with a few varieties, has been the prevailing opinion. At its restoration, and even long after, it was generally believed that there existed a direct vascular connexion between the foetus and the parent, through the medium of the placenta. But the reverse is at present satisfactorily proved. We may indeed affirm that there is no point in anatomy more completely settled. This being the case, it is unnecessary to waste our time by entering into a minute detail of the particular circumstances which render the opinion of a direct communication no longer tenable. Nevertheless, I desire to satisfy all minds, and for this purpose shall state some of the objections which may be urged against it. The alleged vascular connexion between the foetus and parent is disproved 1st, By the total failure of all attempts to detect its existence by injections. 2ndly, By want of corresponding pulsations in the umbilical cord and maternal arteries. 3rdly By the difference between the foetal and maternal blood. To these facts we may add a consideration of great weight, namely, that if as contended for, the blood of the mother enters unaltered into the foetal economy, a transversion of that fluid takes place from

one individual into the vessels of another without having undergone any process of assimilation to adapt it to the peculiar constitution and exigencies of the reciprocal system. Injurious as such an arrangement must necessarily prove still greater damage must result from the propelling of the maternal heart and arteries. Driven by the energies of the organ a stream of blood would crush and reduce it to a chaotic mass, the delicate organ of the embryo or even of the full grown fœtus. Consequences so fatal have been guarded against by a process of nature which I will presently point out. As the direct communication could not be maintained another mode of explanation was resorted to. It was now averred that the nourishment of the foetus was effected in the following manner. The umbilical arteries pour out blood into the cells of the placenta where it is taken up by the uterine veins and having been circulated through the foetal portion is conveyed to the same cells by the uterine arteries. From these it is again taken up by the vein of the umbilical cord and carried through the body of the fœtus fitted for its nourishment. To prove that this is the prevailing opinion at this time I will read you a passage from Blumenbach's Physiology. The doctrine however here announced does not approach nearer to the truth than the former one. The circulation of the uterus and the chord are wholly distinct and independent of each other. The placenta as I before stated is composed

of two parts the foetal and maternal. The first is made up of ramifications of the umbilical vessels; the second of the vessels of the uterus with the interposition of cellular texture. The vessels however of the two parts are more or less blended but they never unite or inosculate. This may be demonstrated by exposing the placenta to maceration and pulling out the vessels which may be separated from the mass without any alteration. Two plants growing near each other in a loose soil whose roots though entangled do not unite or inosculate present not a slight resemblance to the structure of which we are speaking. Hence it follows that the account of the foetal circulation as given by modern philosophers is egregiously wrong. The arteries of the cord have no exhalent outlet but run on without any interruption of their continuity into the corresponding veins so that the blood flows from one into the other without the loss of a single particle. The vessels of the uterus on the contrary have an exhalent outlet or secretory duct through which the fluid destined for the nourishment of the foetus is poured into the cells of the maternal portion of the placenta while the main current of blood is conveyed back in the veins of the uterus. The foetal has a great resemblance to the pulmonary circulation and that which takes place in the maternal portion of the placenta may be compared to what happens in the corpora cavernosa penis. Be this however as it may there is no vascular connexion between the parent and the foetus. The evidence in support

of this fact is clear concise and irresistible. That this is correct may be proved by injection. Let any liquor however subtle and penetrating as mercury, spirits of turpentine &c. be injected into the umbilical artery and the whole will return through the umbilical veins without the loss of a single drop. This experiment was first made by the two Monroes of Edinburgh and by Jno. Hunter and were repeated with the same results in this university. If on the contrary we inject the uterine arteries the uterine veins and the cells of the maternal portion of the placenta will be filled but not an atom will be found in the vessels of the foetus. These experiments have been repeated with the same results too often for us to have any doubt of their accuracy, and they are supported by considerations too important to be overlooked. In the first place it is known that after the expulsion of the infant when we cut the cord no more blood escapes from the maternal portion than what remained in it at the time of division amounting generally to about a tea-spoonful. Second, it appears that the foetus is not affected by haemorrhage from the parent. A remarkable case is recorded of a woman who bled to death and at the instant of dissolution was delivered of a strong and healthy child. Every practitioner has seen the same thing in the profuse bleedings which sometimes attend parturition by which the foetus is not at all affected. Nor is the converse of what I have stated less accurately ascertained; the mother is not at all injured by haemorrhage from the

foetus. In the operation of opening the cranium of the child nearly all the blood of the foetus amounting to several pints necessarily escapes. But the woman debilitated as she must have been by previous suffering (for this operation is admissible only in extreme cases) does not seem to be at all exhausted by this loss of blood which she would be were it detracted directly or indirectly from the system. Thirdly, it has been of late discovered that when by a strong parturient pain the placenta has been expelled simultaneously with the child the circulation continues for some time in the cord provided the child does not respire. But if respiration takes place the pulmonary circulation is established and the other ceases. This fact was first observed by Dr. Rosseau and myself. We made the discovery about the same time. By placing the child in a tub of warm water the umbilical circulation could be made to continue from ten to twenty minutes. A case of this kind happened to two of my students when the period was protracted to more than an hour. They are both men of undoubted veracity and their words cannot admit of dispute. There is also a case of the same nature recorded with great precision in the Medico-Physical Journal of London. Let the fact be admitted (and I can see no reason why it should be doubted) and there is at once an end to all disputation on the subject before us. But additional testimony may be advanced. It is well known that all the lower animals, the monkey perhaps excepted, have not the placenta deciduous as in the human species, but divided into two

portions one of which belonging to the mother is permanent the other belonging to the foetus is discharged at every birth. Now the separation of these portions is never attended with a loss of blood which would be the case were the circulation con=tinuous or carried on by effusion and reabsorption. No one has ever heard of a mare bleeding to death on the de=livery of a colt. Experiments indeed with injections shew that no such connexions exist in this case. There are some animals in which the two portions of the pla=centa have so little resemblance that it is impossible they should be connected in a manner calculated for circu=lation according to the generally received doctrine. Thus in the deer the umbilical portion is highly coloured and very vascular while the uterine is apparently without vessels and of a gelatinous consistence. So in the rabbit one part is of a bright red and replete with bloodvessels while the other is white and shews no signs of vascular organi=zation. To conclude this part of our enquiry I will relate some experiments which I made some time ago and which go to confirm what I have already advanced.

Experiment 1st. I opened the side of a pregnant bitch and divided the umbilical vein as I anticipated the hæmorrhage was profuse and the foetus upon being ex=amined was found to be almost entirely exhausted of blood; repeating the experiment I first tied the arterial cord and no hæmorrhage ensued.

Second. By opening the carotid of a pregnant bitch

I bled her to death. The foetuses were not diminished in size, and the umbilical portion of the placenta contained the usual quantity of blood, while that of the mother was entirely empty.

Third. Knowing that Madder introduced into the system deposits its colouring principle in various parts, I fed a pregnant bitch with food mixed with it. On examining the animal, I found the red colour diffused in different places throughout the body, but no signs of it could be detected in the foetus or liquor amnii.

In the prosecution of these experiments I had various opportunities of observing the difference between the foetal and maternal blood. I found that the former is less florid and exhibits signs of imperfect elaboration. By Bichat the same account is given, and Fourcroy if I am not mistaken, discovered a considerable difference in the results yielded by the two kinds of blood, by chemical analysis. Taking into consideration the whole of what has now been said, I think we are entitled to the conclusion that the foetus fabricates its own blood, and is dependant on the mother only for the materials. This is nothing more than what every one admits to be carried on in the egg. All confess that the chick produces its own blood, and I cannot see why the foetus in viviparous animals which possess the same apparatus, should not be equally capable of furnishing that fluid for itself. By a renunciation of the opinion we have been combating, we are thrown upon the difficult enquiry of what are the uses of the Placenta. To me however, they are not so obscure; and are twofold. First, it is

probable that the blood in passing through the placenta undergoes changes analagous to those effected on the maternal blood by the pulmonary apparatus. this opinion was first thrown out by the celebrated Mayo and was subsequently adopted by the famous Sir Edward Hulty Court physician in the reign of Charles the Second. After this period it was lost sight of until it was again taken up by Doctor Jeffries the present professor of Anatomy in Edinburgh and by Dr. French of Cambridge both of them maintaining the doctrine in their inaugural dissertation which I have never been able to obtain. I understand however that the principal arguments they make use of are the following. 1st. The placenta resembles the lungs in structure and appearance. 2nd. The whole blood of the foetus passes through it. 3rd Compression of the umbilical cord destroys life in the foetus as soon as compression of the trachea after birth. 4th. The blood returns from the placenta having undergone a change from a dark venous into a florid arterial colour. The last of these if well established is conclusive. But as to the fact there is some difference of opinion. By many Physiologists it is denied that such a change does take place. On the contrary it is maintained by equal authority that the fact does not admit of a doubt. Dr Jeffries calls the blood in the umbilical vein "vivida et florida." My own experience teaches me that there is not so

great a change as has been affirmed yet that some change does evidently take place. The circumstance that the blood of the fœtus is not so bright as that of the child after birth arises from the peculiarity of the economy of the former. Surrounded on every side by the proper temperature it requires none of those chemical actions in its whole frame which while they evolve heat communicate at the same time a bright tint to the blood. It may be demanded whence comes the oxygen? Difficult as the question is it is capable of solution. Some provision has been supposed to exist in the placenta like that of the egg by which its blood undergoes a change. But throwing aside this conjecture may we not suppose that in the ramifications of the foetal veins an absorption takes place from the neighbouring arteries of the mother. The foetal and maternal vessels inosculate in such a manner that this absorption can easily be conceived to be carried on. Secondly, besides decarbonating or deoxygenizing the blood the placenta also secretes a fluid for the nourishment of the foetus. It is attested by the highest authority that such a fluid exists in the cells of the placenta in other animals and many celebrated Physiologists affirm that it is to be found in those of women. Harvey Haller Blumenbach Soemerring &c. admit it as a matter of their own observation. It is even termed by Harvey the albuminoid liquor, and in those animals which have their placenta constructed with eminences on one part and corresponding depressions on the other, this fluid, soon after death, may be seen oozing from the

papillæ into the Cotyledons. In the human species it is said to be secreted by the uterine arteries into the cells of the placenta which fact independent of observation is confirmed by the structure of that organ. What other offices can the cells and outlets before mentioned be intended to perform? There is another circumstance which will give us some insight into the end for which the fluid is destined. The connexion between the uterus and mammæ both in health and disease is extremely close. Coeval in their developement these two organs harmonize in all their actions and are distinguished by simultaneous changes. But this is not invariably the case for there are instances where they are all alternately or oppositely affected. The anastomosis between the internal mammary and the epigastric region has afforded one of the most beautiful explanations which has ever been given to any phenomena. I said that the action of the uterus and breasts is sometimes opposite. As examples of this I would mention the suppression of the Catamenia during lactation and the interruption to the secretion of milk so long as the menses occur with regularity. Nor is this all. These organs not unfrequently change their functions. Thus in the state of pregnancy the nourishment of the foetus is committed to the uterus but after the child is born this duty devolves upon the mammæ. In this instance the epigastric artery which during gestation was

large becomes contracted when lactation commences and a determination of blood to the breasts takes place. That the uterus is capable of this secretory office is shewn by the fact that when the milk is suppressed by a cold or by other causes a discharge from the vagina is apt to occur resembling chyle in its appearance. Nor is it less true that when the milk is not secreted within the usual time after delivery the lochia are increased in quantity and of a white colour. These facts I consider as conclusively irresistible and with them I dismiss this part of the subject. By allowing the existence of this uterine secretion we have developed a method of foetal nourishment and the principal difficulty at present is to explain in what manner the fluid is conveyed into the body of the foetus.

Lecture 7th.

By Harvey it was conjectured that this fluid aliment was absorbed by the radicals of the umbilical veins. This is not the case, and it has been accurately ascertained that the vein performs a different function. Nor is it less certain that the power of absorption in veins (if it exists at all) is limited to blood. To me it is manifest that the fluid is taken up by a set of absorbents which open into the cells of the placenta, and running along the umbilical cord, terminate in the liver, where it undergoes a change which accommodates it for entering into the circulation of the foetus. That the liver performs such an office is highly probable from its prodigious size. I am perfectly aware that the hypothesis which I have advanced wants the support of well established facts. There is every reason to believe that absorbents do enter into the composition of the umbilical cord, but no one has ever perfectly demonstrated their existence. Dr. Monro of Edinburgh states that on one occasion he saw the lymphatics in the cord, and a German anatomist went so far as to say that he absolutely injected them. Notwithstanding the assertions of these individuals, I am willing to admit that their observations stand in need of confirmation. But may not the fact that the absorbents have not yet been discovered, be owing to the want of minute examination? Confident that the foetus was supported by maternal blood,

physiologists have not been inclined to search for other means. But because they have not been detected, are we therefore to deny their existence altogether? In fact we have the same evidence that they exist here, as that they do in many other parts of the body. No absorbents have been found in bone, in cartilage, in some parts of the brain, yet their existence in these parts is universally ad=mitted. It is extraordinary that it should ever have been contra=dicted. They are as necessary to the living body and every por=tion of it, as bloodvessels themselves. They are antagonizing pow=ers, and are always found together. Without either of them, growth or reproduction could not be effected. Deprive any part of the body either of absorbents or bloodvessels, and it would inevitably fall to ruin. When we see a certain order of things, the production of which requires a certain agency, we conclude that this agency exists, although no other proof be afforded. The fœtus must be supported either through the umbilical cord, or by means of absorbents. There is no other way by which such an end could be accomplished. If then we shew that it is not effected by the former, we are authorized by all the correct rules of philosophy in referring it to the latter. This reasoning may seem inconsistent with that which I adopted on a former occasion, when en=deavouring to shew that the semen could not be conveyed to the ovary by a set of absorbent vessels. There is a wide difference between the cases, and I am not liable to the charge of inconsistency. The absorbents of the vagina have been distinctly traced, and not one has been seen pas=

-sing in the direction of the ovaria. Besides the objection that lymphatics assimilate whatever is received into their cavities does not apply in this instance. Some alledge that even admitting the existence of these absorbents for which I contend vessels so small as to entirely elude our re-search can answer no such purpose as that which I have assigned them. But we should recollect that as the fluid comes from the placenta perfectly elabrated by the vessels of the mother it has no excrementitious parti-cles to be thrown off and consequently that a small portion only is required for the nourishment of the foe-tus. The reason that we demand so much for our main-tainance is that a great portion of whatever we take in is excrementitious and consequently not adapted for nourishment. Here on the contrary the food has been prepared for the fœtus by the mother and every particle of it when introduced into the foetal system serves for its support. As the process by which the embryo is nourished is well ascertained in oviparous animals let us see what assistance our hypothesis in this case derives from the analogy of the egg. By my own experiments confirm-ed by more than one of the graduates of this university it has been clearly ascertained that about the third day of incubation the umbilical cord of the chick begins to pululate and project forward at the expiration of the eighth day it reaches the folliculis aeris (air bag) at the large end of the egg. The air in this reservoir has

been proved to be pure oxygen by chemical analysis The cord moreover as in viviparous animals consists of two arteries and one vein and the blood which in going is dark in returning is florid. No other bloodvessels enter the vitellus or abdomen. The vitellus or yolk serves for the nourishment of the chick but is not introduced into the system through the umbilical cord. This office is performed by a small duct perhaps of the nature of a lacteal or lymphatic which runs from the ileum or vitellus about one fourth of an inch in length and is called from its discoverer ductus intestinalis Stenonis. Notwithstanding what has been asserted to the contrary the albumen does not serve for foetal nourishment nor is it even mingled with the vitellus. They are separated by the membranes with which each is invested. The uses of the albumen seem to be the same with those of the liquor amnii; both surround and protect the foetus and both gradually waste as that increases. By the vitellus not only is sufficient nourishment supplied for the chick while it is in the egg, but a portion is also left to support it for some time after it escapes until it has gathered strength enough to pick up its food. It sometimes happens that on account of too sudden a contraction of the umbilical cord this residuary portion is excluded and in all such cases the chick dies immediately on being hatched. Let us trace the parallel of the processes in the oviparous animals and the viviparous. In both instances the umbilical circulation which conveys the blood, receives it of a dark and venous, and returns it of an arterial

colour. The resemblance in this respect is perfect. To complete the view, we have only to shew that the nourishment is accomplished in the same manner. In the egg we have seen that the vitellus is subservient to this purpose by means of a duct resembling a lacteal or absorbent, which opens into the intestines. Does not the same take place in viviparous animals? In the placenta there is an accumulation of a milk-like fluid, destined for the same end, and a set of lymphatics must be appropriated for its conveyance, because this could not be done by another agency. On the whole there is a most striking analogy in every leading point, and in this analogy we have presented to us a beautiful exemplification of the simplicity of nature in every important process.

Lecture 8th.

Digestion.

[illegible]

where the resemblance in this respect is perfect. To com-
plete the analogy we have only to show that the nourishment
is accomplished in the same manner. In the egg we have
seen that the vitellus is subservient to this purpose by
means of a [illegible] resembling a [illegible] which
brings into the intestines. Has not the seed of the plant
a corresponding [illegible]? For the plants there is an
accumulation of a [illegible] like albumen destined for the
same end, and [illegible] must be appropri-
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work.

Lecture 8th.

Digestion.

In the preceeding lecture we brought to a conclusion the intricate but interesting history of the foetus. Of its genera=tion, nourishment, growth, and many peculiarities of its econo=my, we have fully treated. Different views now open before us. As soon as the child escapes from the uterus it assumes the con=dition of an independent being, and becomes possessed of a new mode of existence. It is our purpose at present to point out the means by which it is supported. We perfectly well know that on its first entrance into the world, the child begins to feel the necessity for food and drink. But to adapt these for the purpose for which they are intended, they must undergo a variety of changes. Digestion in its most extensive meaning, is the process by which these changes are effected. As you will be taught by the professor of Anatomy the structure of the alimentary canal, and of the organs which are necessary for the performance of this process, I shall leave to him all the details, and proceed immediately to the subject which is about to come under examination. The first step of di=gestion is performed by the mouth; the texture of the food is broken down by mastication, and an intimate mixture with the saliva is effected. But this only takes place in man, and in those animals whose jaws are upon the same construc=tion. Many of the fish, serpents, and birds of prey, swallow the

food whole and the class of animals to which the lion tiger dog &c, belong seem rather to tear than chew it. In ruminating animals as the cow, it is first hastily swallowed and in a short time returned and ground down by subsequent mastication. The stomach of this class of quadrupeds is divided into four parts which communicate with each other and the three first with the oesophagus. The food imperfectly triturated in the mouth descends first into the paunch the first of these stomachs where it is softened by maceration and then slowly passes into the bonnet or second stomach a small and more muscular sac whence after having undergone further digestion it reascends by a kind of regurgitation to the mouth. Here being chewed over again an operation in which the animal takes pleasure seemingly it descends into the maniplus or third stomach (so called on account of the many folds of its internal membrane) and finally reaches the obomasum or fourth stomach where digestion is completed. In Gallinaceous birds or those which live on grain especially the dunghill fowl these organs are also complicated. They have a double stomach the first of which is called the craw a capacious and membranous sac in which the seeds and other food are a little altered in their texture. The alimentary mass here softened by a fluid analogous to the gastric liquor which is copiously secreted by the internal membrane is transferred to the giz-

=zard by the powerful action of which it is ground down and formed into a pultaceous mass. It next enters the duode=num where it mingles with the biliary and pancreatic jui=ces an action takes place and digestion is perfected. No organ is more curious than the gizzard of a fowl. It con=sists of two strong muscles opposed to each other which are covered externally with an aponeurosis and lined in=ternally by a thick hard and polished membrane. The ac=tion of the muscles is oblique slow and steady. Many at=tempts have been made to ascertain their power. But though it is found to be astonishingly great the precise degree is not determined. There is scarcely any substance however hard that is not crushed by their action. The gizzard reduces balls of glass to dust flattens metallic tubes and breaks off without injury to itself the points of needles and lancets. These experi=ments were made by Spallanzani. Notwithstanding this inherent power it is supposed to demand some auxiliary means. For this purpose the fowls collect small pebbles, which are always to be found mixed with the food in their stomachs The utility of these however is utterly denied by Spallanzani who alledges that the operation goes on as well without as with them. He thinks that their presence is owing to accident and to the stupidity of the fowl. To this it is replied by Hunter and Fordyce that the observation is wholly incorrect. They contend that in birds deprived of the assistance of these pebbles digestion is slowly and imperfectly carried on, and in confir=mation of this opinion assert that none are selected except such

as are best adapted to such a purpose. It must be admitted that those best acquainted with the habits of fowls are the poulterers who make it a business to fatten them for market. These men affirm that it is impossible to fat=ten them without allowing them pebbles with their food. The above are only preliminary remarks; not being able to ascertain from experiments on man himself the laws by which the functions are directed, we must therefore re=sort to comparative anatomy and physiology. All the lights we have on many points are reflected lights. It has already been remarked that during the process of mastication the food is mixed with the saliva. Once it was thought to have been demonstrated that this fluid acts as a ferment to the food. But subsequent experiments par=ticularly those of Fordyce have detected the fallacy of the whole doctrine of fermentation. The real use of the saliva appears to be to lubricate the inside of the mouth and the surface of the tongue. Every part of the body destined to convey exquisite sensation is protected by a mucus or a membrane of delicate structure. That the saliva answers this purpose none will deny who have experienced the unpleasant effects arising from its absence or deficiency. The mouth becomes rough the tongue is moved with difficulty and the taste is so obscure that it cannot dis=tinguish different articles. It moreover serves to soften the mass and mould it into such a shape as allows it to be swallowed with the greatest facility; and perhaps

after the alimentary matter has arrived in the stomach it is in some measure subservient to digestion. Thus prepared the food is propelled into the stomach by a combined action of various powers which it would be impossible to make you comprehend without actual demonstration. This part of the business therefore I leave to the Professor of Anatomy. In the stomach those further changes are effected which have always been considered as most important and worthy of attention. The proper stomach of all animals has the power of producing coagulation. Those membranous receptacles in the inferior animals which are merely calculated to prepare the food are not possessed of this power. Thus of the cow none but the obomasum and in birds the gizzard alone are endowed with it. It has not been decided in what this power consists. By many it has been attributed to the power of an acid but there are many substances besides acids which have the power of coagulating certain liquids such as alcohol and the vegetable astringents. Besides it has been proved that rennet steeped in liquid ammonia till it has become perfectly alkaline possesses the power in the same degree as before. By this power then whatever it may be liquid food when admitted into the stomach is coagulated. But this is not the only change; here it meets the gastric liquor which perhaps is the most important agent in the whole process of digestion. Many hypotheses have been advanced to account for the origin of this liquor but they are unworthy of notice. It is now fully ascertained to be a secretion from the arteries of the stomach influenced most probably by

certain glandular bodies which are found suitably situated for such and end. The gastric liquor has often been examin=
=ed and with much contrariety of result as to its properties. There is not any difficulty in procuring it though we often find it mixed with bile and other intestinal fluids. It may be obtained by killing animals which have been previ=
=ously starved for several days. The objection to this method is that the agonies which are consequent upon starvation are so great as to cause a regurgitation of bilious and pancreatic secretions from the duodenum into the stomach. Certain birds as the eagle hawk owl &c may be made use of to procure the gastric liquor. The method generally pur=
=sued is to force them to swallow hollow spheres perfora=
=ted with a great number of foramina filled with sponge which may be withdrawn by a string previously attached to them and left hanging out of the mouth as described by Spallanzani who obtained large quantities of the liquor in this way. According to Spallanzani it is nei=
ther volatile nor inflammable is highly antiseptic and resists fermentation. The taste he says is intermediate between bitter and salt. He denies that it contains either alkali or acid. On this last point writers differ much in opinion. A professor in one of the Italian Universi=
=ties declares that from his experiments he has reason to conclude that in carnivorous animals the gastric juice is acid in herbivorous allakine and in omnivorous neither the one nor the other. By another Italian

of equal estimation it is said that in all animals it is uniformly acid. That it is occasionally so in the human species cannot be contradicted, though in such cases there is reason to be=lieve that it is vitiated by some disorder of the stomach. It seems to me highly probable that it possesses the same properties in all animals and this supposition is confirmed by the fact that carnivorous and phytivorous animals live and flourish on an exchange of food. Distinguished as the gastric liquor is by its other properties its singular solvent power is what renders it so efficacious in the process of digestion. By Reaumur this was proved so early as the year 1752. He forced carnivor=ous birds to swallow metallic tubes which he had filled with meat and after some time pulling them with threads from the stomach he found that their contents were partially dissolved. Twenty years after appeared Mr. Hunter's paper upon the solu=tion of the gastric liquor after death. This turned the attention of physiologists to the subject and gave rise to further inquiry. In 1777 while Dr. Stevens was preparing his inaugural dissertation in Edinburgh there arrived in that city a soldier who was possessed of the singular property of taking into the sto=mach the hardest and most indigestible substances with im=punity. Availing himself of this opportunity he hired the man to swallow metallic spheres perforated so as to admit of fluid entering their cavities into which he put food of the most di=versified description. These in due season were discharged and exhibited such an alteration in the contained food as to afford indisputable evidence that solution had taken place

By Spallanzani a number of experiments were made with the minutest attention on every kind of animals birds fish quadrupeds reptiles and man all of which went to establish the same point. But something more than mere solution is effected. The operation of the gastric juice is a peculiar one; it changes the nature of whatever it acts upon. If the food be thrown up by vomiting some time after it has been taken it will hardly if at all be recog=nized. Putrid matter becomes sweet and tastes and o=dours are completely extinguished by the action of the gastric juice in reducing all articles to an homogeneous mass. The stomach during digestion becomes the centre of fluxion whither the blood is directed in a greater quantity than usual. There is also a concentration of vital energy and an accumulation of animal heat. So great indeed is the latter effect that Dr Rush among other great men taught that the power of creating heat resided solely in the stomach. Experiments have been made to shew the correctness of the statement that the powers of the vital principle seemed to be concentrated in that organ du=ring digestion. At the Hotel Dieu in France a patient who had an ulcer in the leg was made to take food of the most indigestible nature. The skin in conse=quence became flaccid, the ulcer shrivelled and pale and so long as the stomach was employed the man was affected with an aguish sensation not unlike that which takes place in chills and fevers. Nor when the

process is going on is the stomach itself inactive. By its muscular coat a kind of undulatory motion is kept up by which the alimentary mass is turned in every direction in order to facilitate its solution. Nothing has been accurately ascertained with regard to the time which the food remains in the stomach. This depends on the nature of the food the degree of vigour in the organ and the activity of the gastric liquor. At an average however it may be stated at from three to four hours. All the contents are not discharged at once. By a peculiar delicacy of tact or elective sensibility the pylorus only suffers that portion to pass into the duodenum which has been fitted for the purpose. Nor is it less certain that a mixture of old with new food never takes place. To ascertain this point Sir Wilson Philip experimented on an hundred and thirty rabbits. The result was that the new food does not mix with the old in the stomach. Food taken at different times could be readily distinguished into different stages of solution. That portion passing out at the pylorus being perfectly digested while a second was less so and the last portion taken in very little changed. Each portion seemed to form a separate stratum unmixed with each other the last portion however being found in the centre of the contents while the more digested parts remained near the coats of the stomach. As soon as the food escapes from the stomach it experiences a change no less essential than the preceding. The duodenum may be considered as a second stomach. By adverting to the peculiarity of its structure its position the

size and regularity of its curve the valvulæ conniventes which are sensible in its lining membrane and its vast dilatibility we shall clearly see that it was intended to retard the descent of the alimentary matter so as to afford time for the necessary changes and for the lacteals to remove that portion which has been converted into chyle. The soft pultaceous mass discharged from the stomach is denominated chyme from which by an admixture with the secretions of the intestines (the bile and pancreatic juices) the fluid called chyle is elaborated. Most generally it was supposed to be the result of chemical action. The bile is divided into two parts one of which assists in the composition of chyle the other which is bitter and excrementitious descends along the intestines and stimulates them to a contraction by which the faeces may be discharged. But the process of chylification is little understood. It has of late been doubted whether the bilious and pancreatic juices are necessary to it. The experiments of Mr Allen of Edinburgh prove that perfect chyle may be formed though the ducts of the pancreas and that of the liver may be tied with a ligature. These however should not be too hastily received; at any rate we are entitled to demand that they should be repeated with care and accuracy before we yield our assent to a conclusion deduced from them. After this the process of digestion has been considered by many physiologists as complete but it is highly probable that the chyle undergoes further changes

in the lacteals. That every part of the absorbent system is ca=
=pable of acting on what has been taken up is universally conce=
=ded. When we consider the number of conglobate glands in the mesentery the conjecture is still further corroborated for it is diffi=
=cult to conceive for what other purpose they could have been design=
=ed. The change however whatever it may be is slight for chyle has been found in the intestines possessing all the properties which distinguish that fluid. In many points it bears a great resem=
=blance to blood. First it is fluid through life and after death coagulates like blood owing probably to a principle like fibrine. Se=
=condly, There is a portion of the chyle which resembles the serum of the blood coagulating at the same degree of temper=
=ature. Thirdly, One part consists of globules which are sim=
=ilar to those of the blood with this difference that they are smaller and of a white colour. It has not escaped my recol=
=lection that the whole business of chylification has been attrib=
=uted to the absorbents. But few are so bold as to deny that chyle exists in the intestines. The fact indeed is too well estab=
=lished to admit of a doubt. I know of none but John Bell who has ventured to dispute it. It has been found in the duo=
=denum where its presence was owing to regurgitation in the stomach and it has been seen flowing from wounds in the intestines. Having parted with the nutritious portion the mass is next pushed forward into the large intestines. Before it has arrived thus far it is presumed that all the chyle has been absorbed since very few lacteals are to be found in this part of the alimentary canal. This is one reason why the system

cannot be long nourished by injections. But it is not the only one for it is necessary that food should be converted into chyle before it can be taken up in any quantity by the lacteals. When an injection has been administered it generally returns without much alteration or diminution. The chief purpose of the large intestines is to serve as a receptacle for the faeces. Dreadful indeed would have been the consequence if it had been so ordered that they should be discharged continually. The discharge of the faeces however seems to depend principally upon the aliment taken in rather than on nature. Retained until stated periods they give comparatively little trouble and when a call is experienced they are evacuated by the combined action of the intestines themselves and of the abdominal muscles. A warm dispute arose between Astruc and Richerand on the subject whether the abdominal muscles were necessary or auxiliary to the intestines, or whether these were not able alone to perform the office. The latter, after having written a pamphlet on the subject, as if wearied with opposition to a point which he considered so obvious, exclaimed, "Credo Astruccium nunquam cacavisse."

Lecture 9th.

[illegible] has been shown to appear it is not possible not to come to the conclusion that the [illegible] those operations of the animal economy which are under the control of the vital principle. It is no doubt [illegible] of [illegible] it appears that [illegible] the whole [illegible] of the body [illegible] a [illegible] that [illegible] in all its external properties, and that [illegible] perspiration and [illegible] circumstances. [illegible] that the [illegible] by these [illegible] to [illegible] [illegible] But to [illegible] the strong [illegible] of the [illegible] more [illegible] not have been [illegible] other [illegible] than that of [illegible] by [illegible] that the mere difference in the [illegible] of [illegible] [illegible] or [illegible] [illegible] has shown that it is [illegible]. [illegible] by what law of chemical affinity [illegible] can be made of such different [illegible]. What [illegible] of [illegible] [illegible], blood, [illegible], [illegible] and [illegible] [illegible] the same [illegible] chemical action takes place [illegible] the [illegible] of their [illegible] and according to the [illegible]

Lecture 9th.

In reflecting on what has been said relative to digestion, it is impossible not to come to the conclusion that it is one of those operations of the animal economy which are under the control of the vital principle. It is true that from the experiments of Spallanzani, it appears that food may be slowly dissolved out of the body, forming a mass that resembles chyme in all its external properties, and that to a certain extent putrefaction and fermentation are resisted under such circumstances. But admitting this fact, it cannot be denied that the analogy ceases here. No one contends that chyle is formed by these means. Denying the position I assume, we must resort to chemical or mechanical means to explain the process. Not to protract a superfluous discussion, we will only adduce the strong fact of the uniform character of chyle, to prove that it could not have been produced by any other agency than that of vitality. Long ago it was proved by Fordyce that there was no difference in the sensible qualities of chyle as taken from carnivorous or herbivorous animals, and chemical analysis has shewn that it is identical. Now I demand by what law of chemical affinity a similar compound can be made of such different ingredients? What property of chemistry can convert fish, flesh, fowls, fruits, roots, and herbs, into one homogeneous fluid, possessing the same properties and appearances? Chemical action takes place between bodies only within the spheres of their affinities, and according to the nature

of these substances are the results and the results are invariable. Combine an acid with an alkali and you will have a neutral salt and under no other circumstances can this be procured. Nor can we expect more from mere mechanical powers. However much the aggregation shape texture and external appearance may be altered the substance still remains the same. Grind stone into powder; you do not alter its nature. Cut flesh into mincemeat and each piece will retain the same qualities that it possessed before. It is the vital principle that produces the uniformity of chyle. As this is clearly the case it is needless for me to enter upon the old theories of digestion, viz concoction trituration putrefaction and fermentation; each of these hypotheses has been rejected. The objections to them are various and stronger even than those I advanced against chemical and mechanical agency. Digestion cannot be imitated out of the body nor can it be properly carried on in the stomach unless that organ be perfectly sound. Every one knows how digestion is affected by a depraved condition of this organ which occurs in dyspepsia and as in affections of the mind as anger grief deep solicitude, in short whatever emotions or passions strongly agitate the soul. To shew its entire independence of chemical action we need only recur to the well attested fact that if the 8th pair of nerves be divided the capacity of digestion is taken from the stomach. This

was first asserted by Haller and it has since been confirmed by Houghton who repeated the experiment. It is proved by this latter physiologist that if the process be going on it is in=stantly stopt and if it had not yet commenced it never takes place. The same experiment was also repeated by Sir Wilson Philips who found the same result. But it was ascertained by him that by dividing the eighth pair of nerves and apply=ing the galvanic apparatus to the cut extremity so as to ex=tend its influence to the stomach digestion was accomplish=ed as usual. This circumstance led him to consider galvanic and nervous influence the same. In my opinion however this experiment does not prove this point for it is very rational to conclude that the galvanic fluid only acts as a stimulant on the divided nerve and thus keeps up its influence on the sto=mach. Another fact in favour of vital agency is that digestion is carried on with less vivacity in the close of the day than in the morning. Hunter says that rest facilitates the pro=cess. He fed two hounds with similar food and in equal quantities; one he shut up in the kennel the other he hunted and killed them both at the same time. In the one which had been hunted the food remained undigested; in the other it was perfectly digested. It is a common observa=tion made by every one that food taken at night is most op=pressive. This seems inconsistent with the remark just made that digestion is most active when the animal is in a state of rest. But it has been proved by experiments that a diurnal revolution takes place in our bodies and that

all our functions exist in greater vigour in the morning than in the evening. The circulation is more active the pulse being more frequent than in the evening, notwithstanding the stimulus applied to the heart and arteries by the food and drink taken during the day. These circumstances prove that if it is not purely vital it is at least independent of chemical and mechanical laws. The present quantum of our knowledge with regard to this process may be summed up in a few words. It is not to be considered as a simple action confined to the stomach alone but is complicated consisting of a series of processes carried on in different sections of the alimentary canal. In the stomach the food previously masticated and mixed with the saliva encounters the gastric liquor by which it is reduced into a mass denominated chyme. After reaching the duodenum this pultaceous mass is converted into chyle but by what process is not exactly understood. For some time it was considered as the result of a chemical action of the bilious and pancreatic fluids. But unhappily for a conjecture so convenient it was shewn that chyle might be produced though the biliary pancreatic and cystic ducts be tied. These experiments however I told you were not to be implicitly confided in. The only question remaining to be decided is whether perfect chyle exists in the duodenum. That the preponderance of authority is in the

Thomas S. Harper.

Thomas S. Harper

H. Droves.
J. H. Greer
~~[illegible]~~
J. W. Lisle.
T. S. Harper.
E. Parrish.

Students of 1806. in Dr. Kuhn's Office.

[illegible] [illegible]

[illegible]

affirmative must be admitted but there is not wanting some on the opposite side. All however unite in the opinion that the chyle undergoes some change in passing through the lacteals. This is visible by the aid of conglobate glasses. Whoever therefore wishes to clear up all doubts as regards digestion should direct his experiments to that point. But in all our enquiries we are constantly to bear in mind the power of the vital principle and the total inadequacy of chemical and mechanical laws to control the process of digestion.

Absorption.

By a regular transition, we now pass on to the means by which the chyle is conveyed to the circulation This will lead to an history of the absorbent system generally, but first we will treat of the lacteals. By an acute eye there will be discovered while digestion is going on in the duodenum, a number of projecting points covered with a white fluid. These consist of a vein, artery, nerve, and absorbent; which are connected together by the intervention of cellular substance. The absorbents are distinguished by their termination, and by appearances on their coats caused by the valves with which they abound. Commencing at the villi, the lacteals run along the intestines, sending off branches which freely anastomose with each other, and after having proceeded for some distance, turn off at an angle more or less acute towards the mesentery. It is to be presumed that in this tortuous course the chyle becomes more animalized. Having reached the mesentery, they are lost in the conglobate glands. These are small oval bodies consisting of the same elementary parts with the villi, and covered by a polished membrane. When laid open, they may be seen to contain a milk like fluid. Emerging again from the glands, the lacteals proceed, and at last coalesce into the receptaculum chyli or thoracic duct, which is the common trunk or reservoir of the absorbent apparatus. That the lacteals perform the office I have said belongs to them, may be proved by experiment. If

an animal be killed while the process of digestion is going on these vessels may be discovered full of chyle. The same has been discovered in human malefactors who have been opened soon after execution and Cruikshank has observed it in some cases of sudden death. But while all this is confessed it is still maintained by some physiologists that the mesenteric veins also absorb chyle which they convey to the liver. Among the supporters of this opinion is Mr. Hunter. The doctrine is supported by the following considerations. First. If a ligature be applied round the mesenteric veins numerous points of a white fluid are observed floating in the blood which they contain. Second. The blood in these vessels does not coagulate owing to a combination with chyle. Third. They are larger in proportion to their corresponding arteries than the veins of any other part of the body. Fourth. Fluid injected into them passes into the intestines more easily than into the arteries. Fifth. The thoracic duct is smaller than we should suppose that vessel to be were it destined to convey the chyle and the fluids absorbed from all parts of the system. Sixth If the duct be obstructed by a ligature the animal lives longer than we should think possible did this duct afford the only passage of nutriment into the body. These considerations are too plausible to be overlooked. My intention is to notice them in similar order to that in which they have been presented to us. Most of them proceed from incorrect observation.

First. The milky spots are so rarely discovered that they may be considered as accidental and are as often seen in the circulation of the other parts as in the mesenteric veins. Besides the same white spots may be seen in other animals such as geese whose chyle is pellucid as water and cannot give rise to such an appearance. Second. It is utterly incorrect that the blood of the mesenteric veins is not coagulable and if it did not possess that property it would be difficult to prove how the want of it could be owing to a mixture with chyle which is a highly coagulable fluid. Third. This disproportion is not peculiar it is to be found in the spleen and is designed to retard the motion of the blood. Fourth. The experiments of Mr. Hunter shew that fluids injected into the mesenteric veins return promptly by the arteries and that not a particle escapes into the intestines. Fifth. Whether the duct is sufficiently capacious to conduct all the chyle to the heart is a matter that cannot be made the subject of demonstration, but the denial of it is wholly hypothetical. Considering how slowly the fluid is elaborated we have reason to believe that the duct is sufficient for the purpose above mentioned. Lastly. Not much weight is due to the fact that an animal may live a long time when the duct is obstructed by a ligature or otherwise. The cases of this kind are few and not well authenticated. Besides experiments to determine the point are extremely liable to fallacy. To ascertain whether a complete obstruction has been effected must be a difficult matter. But even supposing the duct to be tied up who can tell

how long an animal can live without nourishment? We know that life has been protracted under such circumstances for a considerable period. It has been alledged that those animals on which these experiments are made are apt to have the Thoracic duct double; one running upon ~~the right~~ the other upon the left side. The question of venous absorption was taken up by Jno. Hunter and prosecuted with his usual accuracy and success. He attempted by all the means his fertile genius could suggest to provoke the veins to absorption but was never able to succeed. ~~He never~~ could discover the slightest tendency of that ~~kind~~. It results therefore as well from direct experiment as from presumptive reasoning that there is a distinct set of vessels destined for absorption and he who believes that the same properties reside in the veins must ~~be~~ do so contrary to the evidence of the senses the light of analogy and the usual simplicity of nature.

Of the whole absorbent system the lacteals constitute only a small part. They differ in nothing as to their structure from the rest. But as the chyle is conveyed by them and the chyle only they have received a separate name. Thus also all the others are called the Lymphatics from the nature of the fluid they carry. Like the lacteals they are full of valves frequently anastamose pursue a devious course and become lost in the conglobate glands emerging from which they run to join the common trunk of the absorbents. But as the flu-

=id they contain is transparent they remained undiscovered for a century after the lacteals were made known. During this period the veins were thought to perform the office of ab=sorption, but the claims of lymphatics to this office have been fully proved by experiments. Thus when the bilious duct is tied the lymphatics of the vicinity become tinged with yellow while the veins do not alter their colour. The same happens in hæmorrhages of red blood and in the effusions which are caused by inflammation each of these is taken up by the absorbents only. Coloured fluids injected into any cavity of the body may be traced to the lymphatics. The operation of poisons and of acid substances has been brought forward as a proof of the same point. But this is not enti=tled to much weight for those effects may with more pro=priety be referred to sympathy. But independent of this we have abundant proof that the lymphatics are instru=ments of absorption. The converse of this is not less firmly estab=lished. The notion that veins absorb is at present entirely aban=doned, and even their power of taking up blood is doubtful.

Lecture 10th.

No part of the body is entirely destitute of lympha=tics, but in some parts they are more conspicuous than in others; as in the groin, the axilla, the flexure of the knee and elbow, where the conglobate glands are found of a larger size and in more numerous clusters. Absorb=ents have not been demonstrated in bone, cartilage, nor the substance of the brain, but a few have been de=monstrated by Mascagni in the Pia Mater and at the base of the cerebrum. It has however been doubted whether these were lymphatics, as they could not be tra=ced to any gland in the brain. All the force of this remark is done away by the fact that in many of the lower an=imals these are entirely wanting. Nor is it improbable that the glands in the neck are subservient to the lym=phatics that proceed from the brain. Their size is larger than would be required if they received the absorbents from the external covering of the head alone. Moreover in inflammation of the brain, these glands become infla=med and much enlarged. Be this however as it may, there is strong evidence of the existence of lymphatics in each of the parts mentioned. No one denies that portions of eve=ry section of the brain have been removed, and the struc=ture of cartilages and of bone is manifestly destroyed. Now as this effect could proceed from no other cause than absorption, are we not entitled to the conclusion that

these vessels do exist in these parts. A vast number of experi=
=ments shew that lymphatics arise from every cavity of the bo=
=dy. If coloured fluids be injected into any cavity of the body,
they will be taken up and may be discerned in the vessels.
Nor is it less true that they arise from the surface of the body.
Doubts are only entertained with respect to the two first ex=
=ceptions, the first of which is whether they arise from the
internal surface of the bloodvessels. Cruikshanks believes
that they do, and advances as an argument in favour of this
position that they may be injected from the bloodvessels.
That this is not often done, he attributes to the valvular en=
=trance of the lymphatics, somewhat like the entrance of the
ureters into the bladder. There has existed more difference of
opinion relative to cuticular absorption. Long ago some physi=
=ologists were led to believe its existence, but facts so contradictory
were met with, that there was much hesitation and the sub=
=ject was not brought to a conclusion. The credit of doing this
is owing to our university. About the year 1800 Dr Rous=
=seau of this city began to direct his enquiries to this
subject. By him it was rendered probable that the pulmo=
=nary organs and not the skin are the medium by which
many external substances enter the body. Shutting himself
up in a room which communicated externally by means of
a tube he exposed his body to various applications as Spirits
of Turpentine, garlic, &c. He through the tube breathed the
pure air from without. None of these substances were detected
in his excretions or his blood. On the same principle ex=

-periments were tried well devised and constructed by persons of opposite prepossessions to an almost incredible extent. A candid examination of the whole leads me to the belief that cutaneous absorption does not take place as a natural and ordinary function. Borne down by the weight of authority against them the advocates of the former doctrine had generally yielded their assent to its correctness when the experiments of Dr Mussey revived their confidence. This experimenter clearly proved that if the body be immersed in an infusion of madder this by the proper chemical test (volatile Alkali) might be detected in the urine. Determined to set this question as rest Dr Rousseau assisted by Dr S. B. Smith has subsequently performed a series of experiments to many of which I was an eye witness with every varity of articles; mild and acrid volatile and fixed nutritious medicinal and poisonous. The number of the whole amounted to two or three hundred. The result of these researches was that of all the substances employed madder and rhubarb were the only ones which affected the urine. The latter enters the system most readily. Neither of them were detected in any other of the secretions or excretions or in the serum of the blood. These experiments also prove that the power of absorption is limited to a very small space. The only portions of the body possessed of it are from the middle of the thigh to the hip and from the middle of the arm

to the shoulder. Topical bathing with a decoction of madder and rhubarb poultices made with these substances were applied to the shoulders back abdomen and side and yet no altera=tion was perceptible in the urine. Equally ineffectual were the attempts to promote absorption in the hands and feet. Such is the condition in which the question is left. Though it is not entirely decided yet enough has been done to shew that cuticular absorption rarely happens and never as a nat=ural function. Covered as the whole body is with an impen=etrable cuticle absorption can only be effected in one or two ways; either by forcing the substances under the scales of the cuticle as by friction or by so changing its condition with poultices or bathing as to admit of the transudation of the fluid and its application to the mouths underneath. In no other way do I believe that cuticular absorption ever takes place. Besides the absorption above noticed there is a se=cond species denominated by Mr. Hunter Interstitial absorption. By this in conjunction with the arteries every part of the body undergoes destruction and subsequent re=novation. The condition of the body both in a sound and diseased state affords many illustrations. By this species of absorption the thymus gland is dissipated ulcers are formed tumours discussed and the solids wasted as in marasmus. But the most remarkable effect is the remo=val of the calcaceous matter of bones as in Mollities Ossi=um and rickets; also in the entire destruction of the Al=veolar processes of the jaw after the teeth have been lost

especially in old people. It is needless to produce more facts which might be multiplied to any extent, but there is one which deserves our notice. Most of you know that by feeding animals on madder a deep claret colour is imparted to the bones. This takes place more particularly in animals that are growing and arises from the union of the colouring principle with the calcareous matter of the bones. It has been discovered that after this discolouration the bones at length assume their former appearance. This is owing to an absorption of the calcareous matter and a deposition of new matter in its place. There is not in this case a simple removal of the madder effected for it has been proved that it enters into chemical combination with the calcareous substance of the bones. As to the precise manner in which this process is carried on physiologists are not agreed. By some it has been referred to capillary attraction. To the exercise of this three curious circumstances are necessary. 1st. It is required that the tube should not exceed a certain size. 2nd. It must be of equal calibre throughout. 3rd. One of the extremities of the tube must be immersed in the fluid. Notwithstanding what has been urged to the contrary capillary attraction is not influenced by the flexible nature of the tube nor by its position. It goes on whether the tube is soft or hard vertical horizontal or oblique.

These facts being admitted and that the lymphatics are within the necessary dimensions the doctrine referring absorption to a physical cause is not at first sight altogether unreasonable. Yet if it be narrowly examined many difficulties will oc=cur from which it cannot readily be extricated. First. Did the absorbents act mechanically they would take up all fluids instead of which they use a degree of selection amounting al=most to fastidiousness. Nor on the same principle would the process be retarded or accelerated nor would it be affected by any of those circumstances which we know to have a great influence over the action of the absorbents as com=pression, a state of sleep, and a reduction of arterial action. Second. When a capillary tube is immersed in a liquid the latter is necessarily raised but this is not the case in absorp=tion. On opening an animal immediately after its death we shall find some of its lymphatics empty some full. The process should also go on as well in the dead as the living subject. It has been proved that as soon as the vermicular motion of the in=testines ceases absorption is carried on no longer. This however according to Magendie continues an hour in some cases after apparent death but there is every reason to believe that absorption continues as long. This motion of the intestines may be considered as the ultimum moriens or last remnant of vitality. Third. Absorbent vessels have not the mechanism which capillaries demand. They frequently bulge out in their course again contract commencing at their orifices like fun=nels being very small at the mouth and then expanding. It

has been mentioned by some advocates for the doctrine I have been combatting that the fluid is merely imbibed by this principle and is afterwards carried forward by the contraction of the vessels or of the adjacent arteries. But the hypothesis is not improved by this modification. My opinion is that absorption is not at all under the influence of capillary attraction nor owing to any mechanical cause whatever but is entirely effected by the inherent power of the vessels. How this operates I will endeavour to shew.

When the chyle or other fluid is applied to the mouth of an absorbent vessel this is stimulated to action and thus rendered pervious. The fluid thus introduced is propelled forward by the contraction of the vessels until at length it reaches its ultimate destination resembling the manner in which the foeces are carried along by the intestines and finally egested. This opinion supposes an inherent irritability in the absorbents which has been denied by Mascagni who says that he never could excite them to contraction. This however has not been the case with others. Long ago Haller proved that if touched with a diluted acid or exposed to cold air they evidently contracted. His experiments have been confirmed by other physiologists. The Thoracic duct has been repeatedly brought into view and shewn to possess a high degree of irritability. When pricked by a pointed instrument or touched with diluted acid a weak solution of Muriate of Mer-

=cury or alcohol its contractions may be distinctly perceived. Soemmerring has recorded that in a case of anchylosis the absor=bents became varicose and being punctured discharged their contents with a jerking motion like that of the arteries. If it be difficult to account for the absorption of the fluids how much more so must it be to account for that of the solids. Hunter in the vague manner in which he often expresses him=self says that solid matters are absorbed by a reverse action to that which the arteries used in depositing it. In another place he tells us that they take up solid parts just as a rat gnaws his food. This is no solution and leaves us in as much diff=ficulty as before. By Cruikshanks it was supposed that the solids are previously converted into liquids either by some men=struum secreted from the arteries or by a peculiar action of their own. On the whole this conjecture appears to me most probable. The point however is still in doubt and we must adopt that hypothesis which seems to accord best with the phenomena. But without knowing how it is effected the fact is universally admitted. Except those of the lacteals the mouths of the ab=sorbents are so small as hardly to be perceived. They arise from an orifice of an infundibulum or funnel like shape or by what have been called ampillula. As a similar structure exists in the lacteals we have reason to believe that it pervades the whole absorbent apparatus. But whatever may be the precise mode in which they originate we know that their radices running at first parallel and nearly in con=tact enlarge after a while and coil up into numerous curls

sending off branches which inosculate and anastomose with each other; these reunite and form a network whose meshes are so small as hardly to be perceived by magnifying glasses. This network united with ramifications of the nerves and bloodvessels forms the numerous textures of the body. It is asserted by Macagni that the whole of the membranous tissues as the peritoneum and pleura; and mucous membranes as those which line the trachea mouth urethra &c. consists exclusively of lymphatics. He does not advance this as his mere opinion but appeals to his experiments in proof of his assertions. It is not a little curious that Ruysch renowned for the accuracy of his injections arrived from the same species of evidence at a conclusion direcbly opposite. He maintained that these membranous fibres were composed wholly of bloodvessels. The opinion of neither is correct as exhalation and absorption take place from all membranous surfaces. We require nothing further to prove that both bloodvessels and absorbents exist in them. In cases of inflammation of some of these membranes the arteries may be seen with the naked eye. It is very intelligible how these anatomists were deceived. Each kept his eye fixed on his own particular pursuit. One was celebrated for his minute injections of the bloodvessels the other deluded with no less ardour in the investigation of the absorbent system. Emerging from these cellular tissues the lymphatics unite in trunks so

large as to be readily perceptible. They do not run singly but collected in fasciculi of different sizes some of which are deep others superficial. They are to be found in the greatest numbers on the inside of the upper and lower extremities where they are protected by their situation from external in=jury. The lymphatics of the abdominal parietes are also arranged in two layers one of which is superficial the o=ther deep seated. These vessels are every where so abundant that when successfully injected they seem to enclose the whole body in a kind of network of close and minute meshes. The lymphatics and lacteals lost for a while in the conglobate glands again emerge and send out branches which freely anastomose and being again united terminate by one common trunk, the Thoracic duct, in the left subclavian vein. Thus end all except such as are found on the right side of the head and neck and on the right arm which form a distinct duct which opens in the right jugular vein.

Lecture 11th.

From analogical reasoning we are led to infer that the functions of the lacteals and absorbents are exactly the same. In the early stage of my lectures on the subject, I remarked that the lacteals exercised great influence over their contents. That this is also the case with the lymphatics is indisputable. No matter how various the articles presented to their mouths, such changes are effected that a complete assimilation takes place and they are all converted into one substance. Lymph is as much a homogeneous fluid as chyle. The conversion they ef=fect is as extraordinary as the chylopoetic apparatus. We have seen what a variety of matters, whether the calcareous earth or the fluid secreted by the arteries, as pus, mucus, or serum; sub=stances poisonous, nutritious, or medicinal; animal, vegetable, or mineral; are all converted into a fluid of the same properties on all occasions. There are more points of similitude than one between the chylopoetic apparatus and the absorbents. Whatever articles taken into the stomach prove irritating to that organ are rejected by vomiting. The absorbents do more; noxious sub=stances and those which are not easily acted upon, are refu=sed admittance. By what means this is caused is not correctly understood. These vessels have been supposed by some to ex=ert a kind of elective affinity. But there is no precision in this term. Facts however have been advanced to shew that something like a species of intelligence, or near approach to it, is exercised by the absorbents. When we see bile and chyle

presented to the mouths of the lacteals and observe that one is taken up and the other rejected we can hardly ascribe such discrimination to another cause. The exclusion of certain articles is susceptible of explanation on another hypothesis. It is not difficult to conceive that the mouths of the absorbents may be stimulated to contract and close by the contact of acrid substances or that they may be paraly=zed by narcotics so as to lose all power of exerting themselves or that the fluid offered may be so extremely mild as to be incapable of arousing their action or as happens in other cases lymphatics may require a specific irritation before they can be excited to their wonted exertion. But conceiv=able as this is how shall we explain the preference given to chyle over bile the latter of which when presented to the lymphatics is evidently taken up, yet in the intestines is re=jected. Chyle being necessary to the preservation of life, is admitted by that principle which exercises a controling influence over the whole animal economy, but in what manner I cannot stop to explain. Notwithstanding how=ever many articles escape its vigilance and enter the absorbents. To doubt that this occasionally happens is to be unwarrantably sceptical. But let it not be im-agined that these get into the circulation unaltered As soon as they have passed the barriers the work of assimilation and digestion commences, and they all at length entirely lose their primitive character. Who ever discovered chyle in the Thoracic duct differing in

any of its sensible qualities or its composition? No one as far as I know has opposed the uniform character of chyle. The same re=mark applies to lymphatics of whatever fluid it may be elabora=ted. Interesting examples of this might be adduced. Coloured fluids injected into cavities are in some instances taken up but after a while their colour is lost so that before they reach the receptaculum Chyli their colour is no longer perceptible. It would seem extraordinary that if vegetables which possess only absorbents have the power of assimilation in a high degree, the same power should be denied to animals which possess a digestive ap=paratus. There is indeed no great difference between the absor=bents of animals and vegetables. Each in their course are stud=ded with glandular bodies which have a great share in the process of assimilation. The glands in the human body are ex=ceedingly vascular and more blood is conveyed to them than can be designed merely for their nutriment. Hence we may suppose that a fluid is secreted by them which assists in the digestion of the substance absorbed. It is not unworthy of re=mark that they are large in early life because more nourish=ment is required at that time and all the functions are car=ried on with greater activity. Nor shall we overlook the fact that children affected with diseased mesenteric glands be=come extremely emaciated which circumstance though it may be referred to another cause seems most probably to re=sult from a deficiency in the powers of assimilation. Com=mencing in the stomach and extending to the lacteals and glands, I shall only add that the fact of the glands exer=

cising an influence in digestion can be demonstrated. Coloured fluids taken up and carried to the first gland, when they emerge are found to have suffered a considerable change, and lose more and more of their colour as they pass through the succeeding glands, until they are entirely assimilated. But when there are few intervening glands, as in the liver, the fluid absorbed loses less of its characteristic properties. Hence the bile, when owing to obstruction in the duct it has been absorbed, passes through only one set of glands, and therefore retains the colouring principle to a great extent. Thus arises the yellowness which we observe in jaundice. Chyle is a nutritive fluid, generated principally by the stomach. Digestion and assimilation are common both to the lacteals and lymphatics, but these vessels are not subservient to the same purpose. The lacteals convey their contents into the circulation; the lymphatics remove the excrementitious parts deposited and assimilated by the lacteals. The lacteals to carry out, the lymphatics to return. The one to erect or support, the other to remove or pull down. Thus we see in the animal economy two sets of vessels destined to keep up a constant revolution by their mutual action. The one concurrent and auxiliary, the other antagonizing and opposite; or in other words, the one to perform the office of the architect, the other to dilapidate and reduce to ruins.

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[illegible] in the first place [illegible]
they [illegible] considerable
[illegible] of these where as they
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The Blood.

In the progress of our enquiries, we have now passed over every part of the digestive apparatus by which chyle is elaborated and conveyed to the circulation. The next step is to enquire into the nature of the blood, and into the manner in which chyle is conveyed into it. I hardly need tell you that it is the fluid which is found in the heart and arteries. It is of a red colour, considerable consistence, unctuous or rather saponaceous feel, a slightly saline taste, and peculiar smell. But though generally red, it is not invariably so. In the capillary arteries we often find it white or transparent. Many insects circulate a turbid fluid, and in one kind of butterfly it is found green. Differing so much as regards colour, each of these fluids performs an equal office, and may therefore be considered as blood. If examined immediately after being drawn, it appears to be a homogeneous fluid and emits a halitus which gives rise to the smell which very much resembles that arising from bodies when they are opened. If it be left for a short time to rest, it becomes converted into a tremulous jelly and the surface is covered with a delicate pellicle. This soon separates into two portions, one of which is called serum, the other Crassamentum or cruor. Coagulation and separation are the same processes, the latter being a continuation of the former. In the process of separation, a more close approach of the solid particles, and the expulsion of a fluid called serosity takes place. Blood generally coagulates in three minutes,

though it is often 24 hours and sometimes three or four days before that process is completed. In some instances it never coagulates. The Crassamentum consists of two parts. 1st. The red globules. 2nd. The coagulating lymph or fibrine. The fibrine may be obtained by enveloping a clot of blood in a linen bag and pouring water over it until the colouring matter which is soluble is entirely washed away. The same end may be gained by stirring the blood with a stick in an open vessel. The blood then consists of four parts. 1st. The halitus. 2nd The serum. 3rd The globules 4th. The fibrine or coagulable lymph. Of the precise nature of the halitus we are not informed. It has been ascertained to be the principle which imparts its peculiar odour to the blood and also to the solid parts of the body. In certain disorders it becomes intolerably fetid and is not improbably the medium by which contagion acts. It is soluble in water alcohol and atmospheric air. To the latter in crowded rooms it sometimes imparts a foul and putrescent odour. Whatever it may be it is evidently the product of vitality, as it ceases to be observable after the death of the animal. It has been suggested that it might be some gas held in solution by the blood or the basis of a gas which when it comes in contact with atmospheric air combines with caloric and is set at liberty. But experiments inform us that these surmises are wholly unfounded.

The serum is a murky fluid of a colour between

green and yellow coagulable at 156° Fahrenheit; or more readily by pouring boiling water over it; coagulable also by alcohol and the acids. Chemically treated, it affords albumen, gelatine, hydro sulphurat of ammonia, soda alone and in combination with muriatic and phosphoric acids, phosphate of lime, and according to a late writer, Potash. By bearing in mind its constituent principles, we shall be at no loss to account for the changes effected on it by certain agents. Thus on account of its soda, it turns vegetable blues to green; it is coagulable from the albumen, and by combining with the gelatine, forms a precipitate; digested with the metallic oxides, it is converted into a solid state; probably owing to an union between the oxygen of the metal, and the albumen of the serum. It putrefies when exposed to a moderate heat, by a great one it is decomposed, yielding carbonate and prussiate of ammonia, empyreumatic oils, carbonic acid gas, and the residue is charcoal combined with some substances. The Crassamentum, as I before stated, consists of two parts; the red globules and the fibrine. Some have imagined the red globules to be organized. As relates to their shape, there has been great difference of opinion. By Leuenhoeck and Haller they were thought to be round or spherical. Hewson, who examined them with microscopes, believed that they were flat, having a vesicle in the middle containing a solid particle. This hypothesis has lately been revived and confirmed by Dr Wells, and an Italian

Philosopher who concurred in the same opinion. Cavallo by microscopic observations obtained similar results but he supposed it owing to an optical delusion which most probably is the case. Analyzed, the red globules contain albumen, and perhaps gelatine; soda, phosphate of iron, and other saline substances.

The Fibrine is a substance white fibrous and without taste or smell; it is also elastic. It forms the buffy coat which is sometimes observable in blood; it is the medium of union in wounds and the basis of inflammation. Water has no effect upon it, it forms with alkalies a kind of soap, and the acids act with various results. By heat it is decomposed. Gelatine albumen and fibrine have several points of resemblance which it is important to know, that they may be distinguished. As an ordinary test water will answer our purpose very well. Gelatine is dissolved by it and if evaporated till it becomes solid it is again soluble. Albumen is also dissolved by it but when it has coagulated it is insoluble under 170°. Fibrine is insoluble at any temperature under the common atmospheric pressure. Chemical analysis would furnish us with other means of distinguishing these substances but I have mentioned enough for our purpose. I should not have entered into the above details were they not useful in enabling us to solve a question which shall be the subject of our next lecture.

Lecture 12th.

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Lecture 12th.

The question which now offers itself to our consideration is this: By what means is the coagulation of the blood effected? Coagulation, as applied to this process, is perhaps an improper term. The changes which the blood undergoes seem more analogous to the contraction of the muscular fibre, than to that conversion of a fluid into a solid which has received the name of coagulation. One is a chemical, the other most probably a vital action. Before I give you my own opinion on the subject, I will concisely examine the various hypotheses which have been advanced to explain the phenomena. The well-known fact that blood on leaving the body becomes diminished in temperature, very early led to the supposition that cold was the cause of coagulation. Experience however has taught us that so far from this being the case, directly the reverse is true. Cold, when increased to a great extent, not only prevents coagulation but also destroys coagulability. Experiments by Mr. Hunter prove this to be the case. The same experiments with some variations were repeated by Mr. Hay. By heat on the contrary, the process seems to be promoted. Blood at the temperature of 120° coagulates some minutes sooner than at the temperature of 85°. Cold therefore has no effect in producing the phenomena under consideration. This is a position well established. As the blood which circulates in the body preserves its fluidity, it was very naturally thought that a suspension of motion would produce an opposite effect. This opinion was further confirm=

-ed by the fact that blood loses its fluidity in gangrene of any part of the body in Aneurismal sacs of that fluid and in effusion into the cellular membrane. Coagulation therefore is retarded by motion. We may see this fact illus-trated by agitating blood just drawn in a basin in which are placed a number of small bodies such as peas or marbles. By this plan the process if not entirely suspended may be at least greatly delayed. But a quiescent state does not af-ford a complete solution of the problem. To prove that rest was not the coagulating power, experiments were instituted which you will find in the writings of Mr. Hewson. Among others the following is particularly worthy of notice. He enclo-sed between two ligatures the jugular vein of some animals and for several hours found no disposition in the contained fluid to coagulate. As therefore neither cold nor rest could be considered as the cause of this phenomenon it was next refer-red to the agency of the atmosphere. Whether it really depends on this is not completely decided. Physiologists of the highest estimation have ranged themselves on both sides and each is sustained by circumstances of equal weight though very different in the results that may be deduced from them. I shall endeavour to analyze them and present you with a concise view of the whole.

Blood was observed to coagulate more readily when drawn in a wide and shallow vessel than in a deep one of narrow surface; and the conclusion seemed plain that in proportion as the blood was more or less exposed to the

air coagulation was more or less speedily effected. Not satisfied with this species of evidence Mr. Hewson appealed to experiments. Having laid bare the jugular vein of a rabbit he tied it in three places and having by a puncture let out the blood between two of these ligatures filled the vacant portion with atmospheric air; on the intervening ligature being next removed the blood came in contact with the air assuming the fluid here and immediately coagulating. But this experiment is not entirely conclusive as its correctness has been disputed. Mr. Haighton says that by repeating the experiment exactly as Mr Hewson mentions it he obtained results directly opposite. It is not my duty to decide upon or between such contradictory statements. Emanating from equal authority they hold each other in exact equipoise and do not advance us an inch farther towards a solution of the question. We must therefore wait until more confirmatory evidence be cast into either scale. Hunter utterly denied the power of the atmospheric air in producing coagulation and says that it effects nothing more in this case than any extraneous body whatever. He however goes so far as to assert and rest his opinion on experiments that blood coagulates more readily in vacuo than when exposed to atmospheric air. But to this experiment it has been objected that as he has not repeated the circumstances under which it was conducted we cannot be assured that the vacuum was complete. To remove this objection one of his pupils (Dr Physick) devised and executed a very ingenious experiment. Exposing the jugular vein of a sheep he made an orifice and applied to it a tube with two stop cocks. Opening both

of them he suffered the tube to be filled with blood and then closed them. Upon examination soon after he found that coagulation had taken place. At first sight this must strike every one as conclusive. But it is replied by the advocates for the doctrine of atmospheric influence that the tube was not wholly divested of air but that some adhered to its sides. It is stated that a vacuum existed but there is not sufficient evidence of the fact. Presuming that the air operates by some combination of the blood with its oxygen and not by pressure we cannot decide how small a quantity would produce the effect. This experiment of Dr Physick's is met by one of an opposite tendency made by Mr. Hewson. To an open vein in the arm while the blood was flowing he applied a brass syringe having previously placed around the orifice a piece of wet leather in order more completely to exclude the air. The piston of the syringe was then gradually raised and the blood which followed immediately secured from all contact with the atmosphere. Some blood was afterwards drawn and under common circumstances coagulated immediately or in a short time. On opening the syringe after some time had elapsed about 1/8 of the blood was found coagulated and the rest was liquid. It was urged against the conclusion drawn from this experiment that the portion which had been converted into a solid could not have been exposed to air, and therefore that could not have been the cause of coagulation. But all that the advocates of this

doctrine contend for is a necessary cause and only one out of many others. Considered in this light there is no cause to regret the proposition at any rate the experiment of Dr Hewson demonstrated the fallacy of the opinion entertained by Mr. Hunter that blood coagulated more readily in vacuo than when exposed to the open air. But I do not wish to be thought to adopt the hypothesis of which I have been speaking. My own opinion is that coagulation is a vital operation very analogous to muscular contraction. Whatever influence air rest or temperature may exert they are all subordinate agents. The superior power is derived from the vitality of the blood. But this opinion is not original with me; long since it was advanced by Mr. Hunter who drew a parallel line between the contraction of muscular fibre and the coagulation of the blood with great ingenuity and I think not without success. But it cannot be carried completely through as blood is not susceptible to the action of those stimuli which produce muscular contraction. The analogy however has been so far made out as to shew that when muscular fibres lose their contractibility blood is unable to coagulate. This fact may be illustrated more particularly in cases of death from lightning or electricity from a severe blow in the pit of the stomach from immoderate exercise and I might add from the bite of a poisonous animal. Perhaps it is known to you that when death results from any one of these causes the muscular fibres never contract and the blood never coagulates. To this general rule however there are a few exceptions

which have been stated by Mr Hunter. Independent of this there are other facts which go to prove the vitality of the blood. In the conversion of solid into liquid substan=ces if the laws of chemistry are obeyed an evolution of ca=loric is always the result. In the coagulation no calor=ic is disengaged nor does any change take place in the constituent parts of the blood which would happen if the process was merely chemical. Dr Gordon of Edinburgh and Mr John Davy have had a dispute relative to this point. The former asserted that heat was disengaged during co=agulation but the latter repeated his experiments and obtained a different result. It will not be uninteresting at present to trace the connexion between the vascular action and the coagulation of the blood. The enquiry is important in a particular point of view and moreover may throw some light on the previous question. It was laid down as a general proposition by Mr Hewson and concurred in by Mr Hunter that the action in the vessels and the disposition to coagulation in the blood are in an inverse ratio or proportion to each other. The particular inference drawn from this is that in case of hæmorrhage instead of a cordial and stimulating plan we should endeavour to retain the patient in a state of depression. Correct as is this practice it is not mani=fest that the theory is equally so. By Mr. Hey a directly opposite opinion was entertained. He believed that the tendency to coagulation is always in proportion to

the vigour of arterial action. To prove this proposition he per=
=formed the following experiment. From a sheep as it was slaughtered
he took three portions of blood in three separate vessels, at sep=
=arate times. The first he took as soon as the knife was drawn
from the throat the second a few minutes after and the
third when the animal was in the convulsions of death. The
first coagulated sooner and contained more serum (indications
of a more perfect separation) than either of the others; of
which the last was much slower and had a separation less
perfect. The experiment was repeated by Haighton with the
same results. He opened the jugular vein of a dog and took
5 ounces of blood; in 5 minutes afterwards an equal portion
and before the animal bled to death a third portion of equal
quantity. The results were the same as experienced by Hey.
Hence they say may be deduced a better explanation of the
utility of blood-letting; other methods of reducing arterial ac=
=tion in hæmorrhages. By pursuing this plan we do not has=
=ten coagulation but hinder the impetus of the blood from wash=
=ing away the coagulum of the blood after it is formed. In
conducting these investigations they do not sufficiently dis
=criminate between vigorous and healthy action, and the ex=
=cited action which takes place in disease. As vitality exists
in the greatest degree where the animal is in the plenitude
of health and vigour it would seem a direct conclusion from
Mr. Hunter's opinion that the blood would undergo coagulation
(which is a vital action) sooner in a vigorous than in a depressed
state of the circulation. It is wonderful that this influence

should have escaped the mind of a man so penetrating and that he should have been led into so manifest an inconsistency. But the case is different when marked by an excited and diseased action of the arterial system. By this the blood is modefied in a great va=riety of ways. In some cases the texture of the blood is destroyed so that instead of coagulating it is broken down as physicians say and dissolved. In others we see a buffy coat on the surface of the blood which is supposed to be owing to an alteration of the fibrine so as to allow the red particles more easily to subside. This generally hap=pens in an highly inflammatory stage though it al=so occurs in other states of the system. Fothergill said he observed it in the last stage of Cynanche Maligna and Heberden made the same remark with regard to Putrid Erysipelas. I never drew blood in the Pneumonic form of the late winter epidemic without perceiving more or less of this appearance, and I have been led to believe that it is associated with affections of the chest. Whatever may be the degree or nature of the disease every one knows that the blood of pregnant women has the same buffy coat. This fact is enough to shew that it is not a certain sign of inflammation and should not always be considered as a guide in practice. In some individuals this fact is observable in all circumstances whether of health or disease. From the general scope of my reasoning it may be collected that I am entirely a convert to the

vitality of the blood. Indeed what escaped me relative to coag=
=ulation is of itself sufficient to shew my opinion. There is
no point more clearly made out than this. Every argument
that may be urged in favour of the vitality of the solids will
apply with equal force to the blood. Were it otherwise there
would be a constant contact of dead and living matter
which are hostile to each other. Between the solids and
fluids there is a reciprocal action. Impressions on the solids
will produce coagulation of the fluids and on the contrary
poisons introduced into the blood will occasion the
death of the solids.

As deduced from these views the question arises. Might
not the transfusion of blood from one animal into another be
employed for practical purposes? It would seem that when the
blood becomes contaminated by the introduction of foreign mat=
=ter it might be exchanged for pure blood with advantage.
In this manner also it would seem that old age might be re=
=novated by new blood or that of younger animals. From dif=
=ferent experiments on this subject various results have been
attained. Mr. states that when injected into an=
=imals of the same species it has no injurious effect, but
when injected into a different species it produces death. He
first tried it on two dogs; then injected human blood in=
=to the veins of a dog which ultimately killed him. Exper=
=iments on this point have been too few in number to afford
any decisive results, and I am persuaded that they have
been more unfavourable in consequence of allowing the

blood to stand exposed and thus to lose its vitality. Mr. believes that if used before coagulation no bad effects will result. It is highly probable that if proper experiments could be made the practice of transfusion might be beneficially applied to the treatment of diseases or to the resuscitation of animals from exhaustion and loss of blood.

Whoever wishes to examine more minutely into this subject will do well to peruse Mr. Hunter's papers or ingenious experimental enquiries by Doctor Caldwell of this city.

Lecture 13th.

Respiration.

We have now spoken of the formation of chyle and the chemical and physiological history of the blood. It now remains to speak of the change these fluids undergo in their passage through the lungs. In the prosecution of the enquiry we are opposed not only by the inherent difficulty of the subject but also by many experiments and facts which seem to contradict each other. By respiration we mean that process by which air is inhaled into the lungs and again expelled by the expiratory organs. The quantity taken in at one common inspiration varies so much as to render it impossible to ascertain the exact amount. The average quantity has been stated to be about 40 cubic inches. Every one knows that the air admitted by the trachea is carried through the ramifications of the bronchia to the minutest cells of the lungs. Attempts have been made to ascertain the extent of the surface exposed to its action Such calculations however admit of no accuracy. Con=jecture has made it equal to the whole surface of the body. The blood when it enters the heart is of a dark red colour verging towards a purple. As the chyle enters into the cir=culation slowly and in minute quantities it becomes so inti=mately mixed with the blood as to lose all its distinguish=ing features and except in a few cases is never recognized.

By the Pulmonary artery which arises in the right ventricle of the heart this dark coloured blood is conveyed to the cellular tissue of the lungs where it undergoes a change in colour and becomes of a florid red. Returned in this state to the left ven=tricle it is there taken up by the aorta and conveyed over the whole system. In the smaller vessels it acquires the dark ve=nous colour and admitted to the veins is carried into the heart. That the blood undergoes some change in the pulmonary or=gans was first observed by Mayo about the middle of the 17th century, but Lowyer a succeeding philosopher described the pro=cess with more precision. By the latter writer it was stated that the blood in the Pulmonary veins was florid while in the corresponding arteries it was dark. He ascribed this phe=nomenon to the absorption of some principle from the atmosphere. About a century afterwards Dr. Black by breathing through lime water proved that the expired air contained carbonic acid gas. A long time did not elapse before Priestly who first dis=covered oxygen shewed that the quantity of this is diminished by respiration. The subject was afterwards taken up by Lavoisier and others by whom the previous knowledge was extended and rectified. From their experiments it was ascertained that no animal whether man quadruped or insect whether amphib=ia or fish can live when deprived of oxygen. Even vermes in which life remains longer than in any others will absorb every particle of oxygen in a confined space before they die. But effects so interesting must be traced more distinctly to their causes. Chemical analysis has proved that the atmosphere which was

formerly ranked among the elements consists of three distinct principles. In one hundred parts of common air there are

27 parts of oxygen

72 parts of azote and

1 part of carbonic acid gas.

This is very near the estimate made by Lavoisier whose experiments have been repeated with little variety of re=sult. It must here be remarked that we have referred to weight in this account. Most writers are silent on this point. Berthollet says there are only twenty two parts of oxygen in 100 by measure. By some chemists it has been supposed that as carbonic acid is found in larger quantities near the surface of the earth its exist=ence in the atmosphere is accidental. It has however been detected on the summit of Mont Blanc the high=est of the Alps and hence it would appear to be an uni=form ingredient most probably united with the air in chemical composition. Examined after respiration the air is found to be considerably altered. The oxygen is dim=inished while the carbonic acid is increased and the nitrogen remains the same. Moisture is also given out by the lungs. Experiments without num=ber have been made to determine more accurately the changes undergone but the reports are so contra=dictory that the matter is as unsettled as before. Inde=pendent of the source of error there are many difficul=ties peculiar to this subject which cannot be obviated.

Thus it has been shewn that the results are influenced by the healthy or diseased condition of the system by the food by temperature and a variety of other circumstances. From a comparison however of all that has been reported it appears that really no oxygen is taken into the system as the loss which is apparently sustained is compensated exactly by the same quantity contained in the carbonic acid. This is at present allowed by the most eminent chemists and as oxygen is equal in bulk to the carbonic acid it goes to form and the bulk of air inhaled and expired is about the same their conclusion is probably correct. The quantity of carbonic acid is different under different circumstances and in different individuals. It is increased by food exercise and in the last stage of Intermittents; it is decreased by fasting rest and depletion. I have already mentioned that less of this gas is evolved in the evening than in the morning. It has been before remarked that a small portion of watery vapour is uniformly exhaled with the other products of respiration. Its amount can hardly be determined. That it exists is evident from its condensation when exposed to cold. Murray says that it does not exceed two or three grains in a minute. By the early experiments of Lavoisier we were taught that nitrogen is not concerned in respiration. But an opposite opinion was entertained by Priestly who defended it with his usual pertinacity. Nor was he without supporters, among whom we may rank the celebrated Davy. The experiments however on which they grounded their belief were evidently fallacious. Mr. Ellis says that the quantity is increased when the breathing becomes

more laborious and difficult. The alleged diminution of nitrogen according to him is owing not to an union with the blood but to a retention in the cells of the lungs. This is corroborated by the experiments of Allen and Pepys. They breathed pure oxygen and found that a portion of nitrogen was in the expired air hence they believed that this gas so far from being absorbed is even given out by the blood. We know that a great quantity of air is retained in the texture of various substances and seems to be so by an attraction to the sides of the cells. Thus in the lungs a quantity of nitro=gen may remain adhering to the cellular texture in a condensed state and may be driven off when respiration becomes laborious constituting in this way the increase in such cases. Thus far we have travelled on sure ground. Conducted by the light of experiments we have ascertained with tolerable certainty the changes which the air under=goes in the Pulmonary organs. Two theories have been advan=ced on this subject. The first alleges that the oxygenous por=tion of the atmosphere unites with the hydro-carbon afford=ed by the blood and in this way a great quantity of wa=ter and carbonic acid gas is generated. This was the most obvious explanation and was also supported by analogy. Then if carbon, hydrogen, tallow, fixed air, wax or any other substance having a hydro-carbonous base, be exposed to the action of heat, we have precisely the same result. This being the case, the same process was thought to take place in respiration; or in other words that a slow combustion

was carried on in the Pulmonary organs. Confiding in analogy alone we might be satisfied with this theory, but we should always distrust arguments of this kind. The closer the analogy the greater is our danger of being brought into error. The theory had not long been advanced before it met with objections. It was alledged that the temperature requisite for the combustion of hydro-carbon is greater than that which prevails in the lungs. But this objection has little weight. It is true that to burn Carbon in its uncombined state a greater heat is necessary than any which is excited in the lungs. But the case is different where it enters into a ternary or quarternary compound. The affinity of aggregation is here so much weakened that union takes place between the oxygen and carbon at any temperature. Berthollet's experiments prove this point. He exposed slices of ripe fruit to the rays of the sun and ascertained that the air of the vessel in which they were contained was decreased as to its oxygen and increased as to its carbonic acid. The same takes place from a like exposure of blood. A great variety of experiments have been detailed confirming those of Berthollet. To these it was again objected that as the change was supposed to take place entirely in the lungs the blood in the left ventricle should be hotter than in the right which was found not to be the case. This objection was originally started by Cullen but was completely done away by Crawford. He shewed that the capacity of arterial blood for heat is to that of venous as 115 to 100 and hence requires more caloric to raise it to the same temperature. The heat then is employed in three ways. 1st. A part of it is appropriated to the formation

of vapour in respiration. 2nd. Another to raise the arterial to an equal temperature with the venous blood. 3rd. Another portion is employed in duly heating the expired air. As yet therefore the theory stands unshaken. But we are now approaching obstacles it will be impossible to surmount. Whence come the hydrogen and carbon? It is mentioned by Crawford to be taken up by the extremity of the veins from the solids. But this is not the case for we know that the function of absorption has been appropriated to a distinct set of vessels and it is not probable that nature in this single instance has departed from the general rule An objection not less ingenious was advanced by a late writer Mr. Gardiner. He alledges that if blood owes its florid colour to a loss of hydro-carbon and its capacity for caloric is thus increased by a loss of their hydro carbon must be so completely appropriate the caloric as to prevent its being sensible to the thermometer. Aware of these imperfections in the old theory Lavoisier was induced to abandon it and suggest a new one in its place. The second however on account of the ardour with which they supported it generally goes under the names of and La Grange. Conformably to their views the oxygen of the atmosphere enters into a loose combination with the blood afterwards becomes more completely united and in the course of the circulation gives out heat at the same time that it unites with carbon and forms the carbonic acid. ~~This~~ united with the blood is conveyed into the lungs

and is there given out in respiration. The only difference between these two theories is that according to the first combustion takes place in the lungs while by the second it is supposed to be carried on throughout the circulation. Two things are required to sub=stantiate the latter. First. It must be ascertained whether ox=ygen is really absorbed by the blood in the lungs. This was for=merly admitted but the contrary is now firmly established, no celebrated chemist of the present day entertaining such notions. The second circumstance to be demonstrated is whether venous blood holds carbonic acid in solution. The results of different ex=periments have been different but the most probable conclusion is that there is no such gas in the venous blood. Hence both theo=ries are equally confounded. Against both it may be fairly ur=ged that neither of them describe a process analagous to any other in the animal economy and that neither of them serves satisfactorily for all the phenomena observable in respiration. Now for the various changes which the blood undergoes. The operations of secretion and and assimilation are alike unex=plained. They both suppose an abstraction of air from the blood without altering its composition than which there could not be a more idle device of nature. But the phenomena must be suppo=sed to arise from changes in its alternate composition. Consis=tent with my view of the subject I will offer the following remarks.

The blood is the source whence animal matter is formed. Its expenditures are supplied by accessions of chyle which is a fluid less completely animalized. In the constitution of chyle carbon forms a considerable ingredient and must there=

-fore enter in the same quantity into the formation of the blood. Animal matter contains a large proportion of oxygen hydrogen and nitrogen and but very little carbon. When therefore it is formed from the blood carbon must be left behind in considerable quantities to throw which out of the circulation is of the greatest importance. The only difference between the venous and arterial blood is that the former contains a much larger proportion of carbon. To deprive the system of the superfluous quantity the venous is made to pass through the lungs where meeting with the atmospheric air it gives out its carbon to the oxygen inhaled and is converted into arterial. Carbonic acid is formed from the union of carbon with oxygen and is expired with the azotic gas. Instead therefore of oxygenation we should give the name of carbonization to the change which the blood undergoes in the pulmonary organs. There is no combination of the oxygen of the air with the hydrogen of the blood. It is not necessary to an explanation of the phenomena of respiration and the existence of vapour. It may well be accounted for by the evaporation which must occur from so extensive a surface as that of the lungs at a temperature of 96°. This is the only theory which corresponds with the phenomena. To my friend Dr. Murray of Edinburgh is due the honour of its invention.

Lecture 14th 15.

Before entering more minutely into the consideration of Murray's theory, I will complete the account of respiration and of the production of animal heat. As yet I have considered respi=ration only as a series of chemical actions, but this is not a correct view of the subject. Whatever degree of chemical agency is exerted, it is subordinate only; and as all other functions of the animal economy, so also is respiration controlled by the vital power. The lungs are not passive receivers of air, but have on it a peculiar action. That such is the case is shewn by the fact that if they be inflated with pure oxygen, no more of this gas is consumed than when atmospheric air is inhaled. Of this there is a variety of evi=dence, though no other can be required than that an animal will exist four times as long in oxygen as in an equal portion of com=mon air. But this is not all. Like every other vital function respiration is materially influenced by the nerves. I have already stated that if the eighth pair of nerves be divided or tied with a ligature, digestion is impeded or totally destroyed. By the late experiments of Provencal, we are taught that this operation has the same effect on respiration. The lungs seem at once to lose the power of performing their office, the blood is returned to the heart unaltered, and the animal speedily perishes with all the symptoms of Asphyxia. It is true that the experiments of Brai=die and Legallois are calculated to shew that in this case there is more interruption in the mechanism of respiration. They alledged that by an artificial restoration of the process, the same

action takes place in the blood as before the interruption. But admitting the accuracy of these experiments what do they prove? Nothing more surely than that life lingers in the pulmonary organs. To conclude if the lungs do not exert an influence totally different from chemical action what reason is there to suppose that we could not resusci=tate a dead body by this artificial process. It is conceded that the lungs are only a passive receiver and the changes effected in them are the result of a play of chemical affin=ities; it follows that the operation would be carried on in the dead as well as the living animal provided the organi=zation remains perfect. Connected with respiration and the changes which the blood undergoes in the lungs are the considerations of animal temperature. Nothing earlier attracted the attention of mankind than the power which the more perfect animals possess of preserving an uniform temperature in every vicissitude of heat and cold. The ancients treated this with great reverence; they consid=ered animal heat as a direct emanation from the Deity and as the principle of vitality. As Science advanced it is not surprising that many attempts were made to explain the phenomena but they do not deserve our notice. Before the late brilliant discoveries in chemistry all with respect to animal heat were absurd speculations and founded on weak analogy. Mayo indeed attributed it to changes in the blood but his notions were very vague and indefinite. The discovery of Dr. Black was a primary step towards a correct under=

=standing of the subject. To Crawford we owe its full elucidation and the establishment of a just hypothesis. During respiration ox=ygen combines with the carbon of the blood a species of slow com=bustion takes place and caloric is evolved. The blood by losing the carbon is changed from venous to arterial and acquires an increase of capacity for caloric in the proportion of 115 to 100. By this in=crease of capacity the blood is enabled to take up the caloric dis=engaged from the oxygen and this prevents its detrimental effects on the lungs. The arteries now convey the blood to every part of the body where it is converted into venous and of course its ca=pacity diminished as much as it was previously encreased. The caloric is evolved and being distributed over the whole system preserves it of an uniform temperature in every part. The superiority of this theory consists in a discovery of a difference between venous and arterial blood. All for=mer hypotheses ascribed it to a disengagement of heat in the lungs alone but none explained why the temperature was not higher than in any other parts of the body. Crawford's theory is still defective in this point, that it supposes only a single source of heat. Besides that of the lungs there are several others which ought not to be overlooked. Of these the skin is the principal. Experiments by Cruikshanks shew that the air in contact with the surface undergoes changes similar to those which are ef=fected in respiration. A portion of the oxygen disappears, and car=bonic acid gas is formed in its place. It is averred that the con=sumption depends on the quantity of blood directed to the cuta=neous vessels, and therefore is increased by exercise and a high

temperature. As oxygen is taken into the stomach with the food caloric is also evolved in greater or less quantity during the process of digestion, and in a slight degree separate from the air which is in contact with mucous surfaces. But the amount of heat resulting from these three sources is small in comparison and is consumed as fast as it is given out; on the surface it is employed in causing the vapour of insensible perspiration in the stomach it unites with the aliment converted into a liquid, while that produced by the mucous surfaces is consumed in rendering the secretion thinner and in evaporating it. An infinitely greater laboratory is to be found in the processes of the body by which the caloric is brought from a latent to a sensible condition. We have seen how much is evolved from the arterial blood when it undergoes the change into the venous. Though the other sources are not so evident, yet their existence cannot be doubted. Without entering into minute details I will mention only those which are most obvious. Heat is developed during the formation of solid matter from the blood. Thus we infer that solids have less capacity for caloric than liquids. It is also set free by the secretions. During the operation by which they are produced a more intricate mixture takes place between the carbon and the oxygen and the latter of course gives out a portion of caloric. To this rule there are two exceptions. In the secretion of bile and fat combustible matter is abstracted from the blood. Heat is also increased by exercise

but whether this is owing to an increased flow of blood or some change in the muscles themselves it is difficult to determine. I have now enumerated the chief sources in the body whence heat is derived. But the body under all circumstances whether exposed to polar snows or tropical heats preserves the same degree of tem=perature. Let us now trace the means by which an accumulation of heat in our systems is prevented. The temperature of the body being higher than that of the surrounding medium these means are very intelligible. We well know that caloric passes from a hotter to a colder substance until they both acquire the same degree of temperature. To this may be added the extensive evaporation which takes place from the surface and from the lungs. Be=sides the air we inhale being cold when it enters the lungs and hot when it leaves them must have taken up some of the heat and of course is another mean by which accumulation is prevented. The caloric thus removed amounts in twenty four hours to as much as could suffice when the atmosphere is of the temperature of 59° to melt thirty pounds of ice. As an auxiliary means assimilation may likewise be mentioned. By this process the food being wrought into a fluid must have a greater capacity for heat and appropriate of course a large proportion to itself. The degree in which heat is taken from the body is regulated by the following circumstances. 1st. By the greater or less difference between the temperature of the body and that of the medium in which it is placed. 2nd. By the conducting power of the medium; (as for instance, metals conduct more rapidly than glass, glass more rapidly than charcoal, wool

and cotton are also bad conductors.) 3rd. By the spontaneous motion of the medium. It has been ascertained that a stream of water or air draws off heat faster than the same does at rest; dry or moist air has the same effect. In the former it absorbs the perspiration and thus converts a liquid into an air; in the latter it withdraws heat by the conducting powers of the watery particles. It has been remarked by Boerhaave that no animal can live in a temperature higher than its own. But that this is not the case has been demonstrated by the experiments of Tillet Fordyce and Seyden. The two latter shut themselves up in a room heated to 215° which was gradually carried up to 260° for 15 minutes and one of 280° for 16 minutes without being much if at all altered in his own temperature. In the same room meat pla=ced beside them was completely cooked and bread introduced in the form of dough was in 3 or 4 minutes incrusted as if baked. It is worth our while to enquire into the causes of such curious events. I have already noticed that evap=oration and perspiration are causes of a diminution of temper=ature. In the cases I have just mentioned streams of water flowed from their bodies and if I am not mistaken the loss of weight in one person was 10 or 15 lbs. in ten minutes. But other causes conspire to keep down the heat of the sys=tem. It has been proved by Crawford that a much smaller quantity of oxygen is consumed in a high than in a com=mon temperature. He also shewed that when the body is exposed to a very high temperature the change from arterial

into venous blood does not take place. It is also probable that under such circumstances the blood acquires a vastly increased capacity for caloric. Such then are the causes which counteract the increase of heat in the surrounding medium. In a high temperature little or no oxygen is taken into the system, and none of the changes go on which evolve caloric; and evaporation from the surface and lungs by con=verting a liquid into an air serves to abstract a vast quantity of heat. Complete however as the theory of Crawford appears to be in all its parts it has not been without assailants. The most conspicuous of these was the celebrated John Hunter. The objections he brings for=ward are worthy of particular notice. In the contusions of the head he observes that respiration does not take place more than 5 times in two " minutes yet the temperature is not at all diminished. To this we answer that we have been left in the dark with respect to the circumstances of the case. We are told neither the temperature of the place where the patients hap=pened to be nor the state of their respiration yet all these are neces=sary to a proper discussion of the subject. 2nd. He says that in apoplexy the patient is hot and cold alternately though no corresponding change is observable in the pulse and respira=tion. But we may explain this by supposing that the variati=on of temperature was owing to a change in some of those processes by which heat is generated in the body independent of any direct communication with the lungs. 3rd. When large arteries have been taken up in a limb as in the opera=tion of Popliteal Aneurism the temperature of the limb is not always diminished but on the contrary is sometimes augmented.

That such is the case cannot be doubted. Others have noticed the same thing among whom we may mention Dr Physick. But still the circumstance is of rare occurrence so much so indeed that it may be considered as an anomalous fact rather than as a general rule. The temperature is commonly so much diminished that warm applications become necessary. When such objections are brought forward to refute a received opinion all the circumstances should be detailed with accuracy. But this has not been done in the present instance. It is important to know the precise state of the circulation in the part; sometimes no diminution in the circulation takes place and I have even known it increased. This may be accounted for by considering the anastomosing vessels which when the main current of blood is obstructed are almost always enlarged and often an equal quantity of blood may be sent to the limb to that before the operation was performed. To this we may add that by the violence done to the part its condition may be so essentially changed that the process by which caloric is separated from the blood may go on with greater force. We have before remarked that animal temperature is derived from many sources independent of the lungs. By not attending to this many futile objections have been raised against the theory of Crawford. A much more formidable one arises from the late experiments of Mr. Brodie. He has lately shewn in a series of well-conducted experiments that in the animal where

the brain has ceased to perform its functions either from decapita=
=tion or other causes if artificial respiration be induced all the
changes will go on in the blood as in a state of health yet no
animal heat is generated but on the contrary the animal is
colder than usual on account of the cold air thrown into
the lungs. These experiments if true go farther than any
thing which has hitherto been advanced to overthrow the the=
=ory which has been engaging our attention. I do not however
consider them as complete. The human mechanism is exceed=
=ingly complicated and if any part be interrupted the result
of the vital actions must be different from what they are in
a state of health. The strongest ground on which the chemi=
=cal theory rests is that in all animals the temperature is
proportioned to the quantity of air taken into the lungs.
Thus birds, which have large pulmonary organs in proporti=
=on to any other class have also a temperature of 4° or 5° high=
=er. Next are man and the more perfect animals whose lungs
being large admit much air and of course a higher degree
of heat is generated. But in amphibious animals whose lungs
though large expose a small surface to the action of the air
the temperature is much lower and indeed not much exceed=
=ing that of the medium around them. In hybernating animals
where the respiration is suspended or carried on very slowly and
at long intervals the temperature is proportionably diminished.
When all these facts are compared we seem entitled to the in=
=ference that animal temperature is owing to the change which
the air undergoes in the lungs and to other processes carried on

throughout the body; all the operations however being un=
=der the controul of the vital principle. From experiments
on this subject on proper consideration I am inclined to
abandon the chemical theory of animal temperature.
Curious as it may appear it has been ascertained of
late that the blood possesses no carbon. This experiment
was tried by a graduate of this school two years ago.
On analyzing the blood the chemists themselves could
detect no carbon in it. This has never before been taken
notice of by any writer. I have to lament that our
knowledge on this subject is so imperfect. I can offer
you nothing positive upon it. I will content myself
at present with clearing away the rubbish and prepa=
=ring the site; to your future labours I resign the erection
of the superstructure.

Lecture [illegible].

Circulation.

[illegible]

throughout the body, by the [illegible] being in
on the [illegible] of the vital principle. [illegible] appears to
[illegible] subject [illegible]
[illegible] the [illegible] of mental [illegible]
[illegible] appear to [illegible] of
[illegible] This experiment
was done by [illegible] years ago.
[illegible]
[illegible] before him [illegible]
[illegible] that [illegible]
knowledge on this subject is so imperfect [illegible]
you nothing [illegible]
[illegible]
[illegible]
of the experiment.

Lecture 15th.

Circulation.

In the progress of our enquiries we have reached that stage of the course in which it will be proper to describe the circulation of the blood. The term has been applied to that function by which the blood is carried to and from the heart. It is of vast import=ance in the economy of animal beings. It conveys the blood to the lungs where it undergoes the process of respiration to the glands where it furnishes matter for secretion and in fine distributes nourishment and animal temperature over the whole body. To comprehend the nature of the circula=tion it is necessary that some general description of the organs subservient to its performance should be premised. These consist of the heart arteries and veins. The first is a mus=cular sac contrived so as to expel into the arteries the blood which it has received from the veins. But its structure would be very simple were its only office to expel the blood into the arteries. One auricle and one ventricle then would be sufficient. But the blood is unfit for the various ends it has to perform in the system until it has been ren=ovated in the pulmonary organs. The heart then is divided into two parts one of which is appropriated to sending blood into the lungs, the other to its distribution over the body and thus a double circulation is carried on. The more complete hearts have two auricles and two ventricles. These

in the human subject are placed in contact but in ma=
=ny of the inferior animals they are distinct and indepen=
=dent organs being separated to some distance from each
other. One is subservient to the general the other to the
pulmonary circulation. In describing the circulation
it may be said that the blood through the vena cava
enters the right auricle by the contraction of which it
is forced into the corresponding ventricle when con=
=veyed by the pulmonary arteries to the lungs it is
returned by the pulmonary veins into the left auri=
=cle. This contracting empties it into the left ventricle which
by a vigourous impulse sends it into the arteries through which
it is every where diffused. Carried into the minute radicles
of the arteries it passes into corresponding radicles of the veins
which uniting in the main trunk the vena cava restores
it to the right auricle. I have remarked that by the con=
=traction of the left ventricle the blood is impelled into the a=
=orta but it would flow back into the left auricle were there
not some contrivance to hinder it. Accordingly a valve is
placed between these two cavities which from its supposed
resemblance to a mitre has been called Mitralis. For a
similar reason a valve is also situated between the right
auricle and ventricle and has received the name of tri=
=cuspis. There are besides these two other valves; one at the
mouth of the Pulmonary artery the other at the commence=
=ment of the aorta which from the shape are called Semi=
=lunar. Their object is to hinder the regurgitation of the

blood when these two arteries contract on their contents. It appears that the bloodvessels emanate from one point at the heart and reunite at their minute ramifications. But as to their precise mode of connexion there is not the same co=incidence of sentiment. By the illustrious discoverer of the circulation it was taught that the blood was effused from the arteries into the small cells from which veins originate and take it up. But this opinion has been contradicted by micro=scopical observations. Examined by powerful magnifying glas=ses the arteries of some animals as in the web of a frog's foot or the ear of a rabbit may be seen running into veins with=out any dilatation of the part where one ends and the other commen=ces. Nevertheless we have reason to believe that particular organs afford specimens of the structure mentioned by Harvey but these must be considered as exceptions to the general rule. Thus it appears that the cells exist in the spleen in the corpora ca=vernosa penis in the clitoris and papillae of the breasts of wo=men. Even this however is contradicted by some Physiologi=cal writers. Besides the two already mentioned there are sev=eral other modes of termination. In glands a secretory duct runs off laterally from the arteries and the main current of the blood continues on into the veins. Arteries are distributed on the surface of the body on the lining membranes of the va=rious cavities and every where throughout the cellular tissue and open by outlets which have been denominated exhalents The existence of exhalents has absolutely been denied by two of the most eminent physiologists. Dr William Hunter believed

that the effects ascribed the the exhalents were owing to a mere exudation through inorganic matter and Mascagni enter=tained the same opinion as regards both the arteries and lymphatics but they have not advanced much in support of their peculiar notions. At the same time it must be con=fessed that no one has ever demonstrated the existence of exhalents though microscopic observations have been dili=gently made. But in as much as there is no evidence that transudation ever takes place in a living body it is impossible to account for exhalation any other way than by the opinion which now generally prevails. Many branches of the capillary arteries are two minute to admit the red globules. Of this we have examples in the medulla=ry part of the brain in cartilage tendon ligament or the tunica conjunctiva of the eye and in all the white or transparent tissues which are formed throughout the body. Yet as these parts grow like the rest of the system it is pre=sumed that they are nourished by serous vessels. This may be demonstrated by injection and the phenomena of disease. All of you know that when the tunica con=junctiva is inflamed the vessels previously transpa=rent become filled with red and opaque blood. The capillary vessels do not constitute a distinct vascular system but are to be considered on the one hand as the minute radices or beginning of the veins. They are inconceivably numerous so much so that when injected by Ruysch they seemed by their mosculations to form

a network over the whole body. What stronger proof need we advance of their extreme minuteness and vast number than the fact that hardly any part can be pricked with the point of a pin without the escape of blood? The heart is an irritable organ and is put in motion by its own proper stimulus the blood. No other fluid can excite it to a proper action. Even of the blood the heart demands a certain portion only for the performance of its functions. When over destended it is incapable of action and hence the circulation has been stopped by violent action strong passions as in the case of Hunter and by any other cause which in a great degree determines the blood to the heart. The same effect may be produced by an undue detraction of blood. Every practitioner has seen this exemplified in cases of Syncope during copious venesection or from loss of blood by hæmorrhage. Not a little has been said at different times relation to the degree of power which the heart exercises in the circulation. The difficulty of determining this is strikingly illustrated by the total disagreement in the estimate of different writers. Kiell makes it amount to a few pounds only, while Morelli considers it as equal to 180,000 lbs. Before we engage in any calculation respecting the matter the following data should be clearly established. 1st The quantity of blood expelled from each ventricle at every contraction. 2nd. The degree of velocity with which it is expelled. 3rd. The amount of resistance each ventricle has to overcome before it can propel the blood into the corresponding arteries. 4th. The effects of the action of the heart on the blood. But

these are points which most probably never will be ascer=
=tained with any sort of precision and computation must
of course be vague and conjectural. All we know with
certainty on the subject is that the heart is a muscle of
great strength as is evinced by the phenomena of circula=
=tion; and further by the fact that if the heart of a li=
=ving animal be grasped in the hand no effort will re=
=press its action. The experiment was tried by Mr. Haigh=
=ton of London and I saw this evinced. An enquiry
far more interesting here presents itself. Why as has
been frequently asked does not the heart become ex=
=hausted by exertion like the voluntary muscles?
Many answers to this intricate question have been
attempted; no one however entirely satisfactory. I will
not detain you by relating the idle hypotheses of Des=
=cartes Scelvi Bonnet &c. Even those I shall introduce
to your notice owe much more to the celebrity of their
authors than their own intrinsic merit. 1st. By
Stahl it is altogether imputed to the intelligence of
his anima medica. A guardian power foreseeing the
danger to which they would be exposed by a remission of
the action of the heart ordains it otherwise. This is
pure hypothesis of the most wretched kind by the adopt=
=ion of which we only cut the knot we cannot untie.
What is more opposite to our idea of an intelligent prin=
=ciple than that it should direct operations of which
it is wholly unconscious? Is it to be supposed that every

insect every worm that crawls upon the ground should foresee the consequences of interrupting the circulation; a function which is unknown to an infinite majority of the animated beings in whom it is performed? Nor can there be any thing more unphilosophical than to seek for an explanation of physical phenomena by the interposition of the intelligent faculties. 2nd. By Willis it is maintained that the voluntary muscles derive their nerves from the cerebum while the cerebellum supplies the heart and all the other involuntary muscles and hence he infers that the one set is fitted for temporary and the other for permanent and uninterrupted action. Till the middle of the last century this theory was received without modification. Boerhaave suggested some additions. He conceived that the action of the heart was excited by the blood in the coronary vessels and cavities of that organ. The whole hypothesis is predicated on premises wholly incorrect. No one has shewn that the two sections of the brain possess this difference on the contrary the distribution of the nerves which arise from both is indiscriminately distributed sometimes to the voluntary and sometimes to the involuntary muscles.§ The third theory was that of Haller. Every muscle he conceived to be endowed with irritability. This varies in different muscles not only a regards degree but also in not being obedient to the same stimuli. The nervous influence is the natural stimulus to the voluntary muscles, and through this medium they are called into action at the instigation of the will. It is otherwise with the involuntary muscles. Callous to the operation of the will

§ By an error of the transcriber the 15th & 16th lectures are united The 15th ends here.

they obey their own appropriate irritant only. Of the heart the proper stimulus is the blood. From these premises it follows that the motion of the heart is unceasing because its irritability is inexhaustible and because blood is constantly supplied. These are the only conditions necessary to perpetuate muscular motion. To prove them he alledges, 1st That though the communication between the brain and heart be interrupted the action of the heart is still retained. 2nd. That though taken out of the body it continues to contract for some time. 3rd. Though the nerves going to it from the Medulla Oblongata and spinal marrow be irritated the heart is not at all affected. This hypothesis however beautiful is liable to a great many objections and is subverted from the foundation by the experiments of Legallois. As the result of the whole that has been done on the subject it may be stated that in an animal which has been beheaded the action of the heart does not cease immediately on account of the removal of the head but that its cessation is an indirect effect of the suspension of respiration. It has even been proved that though the head be taken off yet if the lungs be artificially inflated the action of the heart is restored but if the spinal marrow be destroyed the heart loses entirely its power of regular contraction. The inference is plain the spinal marrow and not the brain is the source of motion in the heart. It may further be inferred from the experiments alluded to that the motion of every

part depends upon that section of the brain or spinal marrow from whence the nerves supplying it are derived. The question stated opens a wide field for discussion but to pursue it farther would be to anticipate. All that I shall add at present is that there is nothing in it which should confer on it the power of perpet=
=ual motion. Endowed with irritability and susceptibility to the influence of the blood it has regularly its systole and dias=
=tole. The one occasioned by its appropriate stimulus, the other by that tendency which all muscles have to relaxation after they have been exciting themselves. That blood is the proper stimulus of the heart may be proved by the fact that the left ventricle ceases to contract first because first deprived of blood. This is owing to the difficulty of circulation through the lungs while the opposite side is copiously supplied by the Ve=
=na Cava. The heart is also influenced by the condition of the coronary vessels. It is well known that if venous blood gets into the left side and into the coronary arteries death is the inev=
=itable consequence. Mr Goodwin endeavoured to explain this by supposing a want of power in the venous blood to stimu=
=late the heart. But this will not apply to the right side which receives no other than the venous blood. The expla=
=nation here fails. Mr. Goodwin's theory of course failed and the subject remained unaccounted for until Bichat's theory ap=
=peared. He stated that death is not occasioned by a want of power in the blood to stimulate the left ventricle but by its getting into the coronary arteries. Like all other organs the heart is supported by its appropriate vessels and the want of

a necessary supply of arterial blood in them as well as in other cases destroys its power of action. The arteries are conduits or tubes conveying the blood over the whole body. Beginning by a large trunk (the Aorta) in the left ventricle and sending off branches almost infinite in number they are not inaptly compared to a tree whose root is in the heart and whose numerous branches extend in every direction. Their size decreases the farther they proceed from the heart They are not however tapering or conical but rather cylinders gradually diminishing. As the diameter of all the arteries taken together exceeds that of the trunk their capacity is increased the further they proceed from the heart. Their direction is often tortuous and this arrangement is particularly obvious in the hollow viscera as in the heart uterus stomach and bladder and in those parts where much distension is requisite as in the cheeks, &c. As they proceed from their primary source they form anastamoses in several ways. These are sometimes in the form of arches as in the mesentery sometimes the vessels join at an acute angle as in the basiliary artery and sometimes they are united by branches running directly transverse as in the brain. The end of either of these modes of arrangement is according to circumstances either to retard or accelerate the circulation. The arteries have three coats the external is cellular and elastic the middle one fibrous and the innermost beautifully polished and serving as a lining to the others. There has

been much discussion about the muscularity of the arteries. Haller says that the small ones do not possess a contractibility and the larger ones only in a slight degree. But he is wrong. Every portion of the arterial system contracts. In the larger vessels it may be seen with the naked eye and the contractibility increases as they proceed from the heart. The capillaries possess muscularity in a greater degree than any of the others This was first remarked by Cullen, and was afterwards confirmed by Jno. Hunter. The muscular fibre cannot be demonstrated on account of the minuteness of its vessels. But they contract (and this power resides in muscles alone) independently of the heart. But it still is a question whether the heart alone carries on the circulation or whether it is assisted by the arteries. Harvey maintained the former opinion and has not been without followers. Granting to the heart the chief agency in this business it is also true that the arteries exert some power, in general auxiliary to the heart, though sometimes independent. Numerous facts may be adduced in confirmation of this assertion. 1st. Muscular energy resides in all arteries large and small in the main trunk and in the extreme capillaries. 2ndly. As the arteries contract with considerable force the unavoidable effect is to propel the blood. 3rdly. The circulation has been kept up in the foetus though entirely destitute of an heart. Cases of this kind have occurred too often to leave any doubt of the fact. Nevertheless it may be alledged that in those cases as often happens in the animal economy that the want of one organ is supplied by an increased

energy or an entire change in some other. I do not see much force in the objection but as it might be advanced I will appeal to other facts which are of themselves sufficient to shew the independent action of the extreme vessels. It is however by the phenomena of local inflammation of active hæmorrhage of blushing and of hectic suffusion sufficiently proved. Enough has been said to shew that the circulation may be increased in particular parts without any general effects; consequently, that the arteries may sometimes act independent of the heart. The Pericardium and ventricles have been found ossified and the heart in different ways impeded in its action and yet the circulation has gone on well in the arteries.

Veins are those vessels whose office it is to carry the blood back to the heart. In their general construction they resemble the arteries and like them have three coats but they are thinner and not so strong. They are more numerous than the arteries. In some places each artery has two corresponding veins besides others seated in superficial parts. An idea may be formed of the general disproportion between the two sets of vessels from a computation that of 28 or 30 lbs of blood (the medium quantity contained in an adult) nine parts circulate in the veins and only four parts in the arteries. Veins have another peculiarity; I allude to their values. But this structure prevails only in those which are exposed to lateral pressure. The use of the valves has been supposed

to consist in preventing the weight of the column of blood from pressing on the inferior parts but the real advantage is the obstruction they afford to the retrograde motion of the blood. This is proved by the fact that deep seated veins and those which come forth from the viscera are entirely destitute of valves. Most generally veins accompany the arteries though there are some exceptions to the general rule as in the brain liver and bones. As muscular fibres are not very conspicuous in the veins it has been asked how is the blood conveyed to the heart and arteries by the original force? But their impulse is lost in the capillary vessels or at least is not sufficient to raise such a column of blood. There are I think three causes which operate to produce the end proposed; and the most efficient is the contractile power of the veins themselves. I am aware that this property has been denied them by many who have speculated on the subject. It has however been shewn by Haller that the vena cava are muscular, and
and other respectable physiologists have detected the same structure in more minute vessels. There is one circumstance which of itself should convince of the contractibility of the vessels; they always adapt themselves to the quantity of blood they contain. Co-operating with the above cause is the action of the muscles, which may be illustrated by the familiar operation of venesection. When the blood issues languidly from the orifice, nothing promotes the flow more effectually than grasping something firmly in the hand. This operates by bringing into action the muscles of the fore-arm and humerus

which compress the vein. An auxiliary means of propelling the blood may be found in the action of the neighbouring arteries. This however is a slender assistance and the two first are the principal agents in the business. In addition to these the veins are straighter than the arteries and their column is broken by the valves. Compared with the mechanism which produces the arterial circulation these three causes are extremely feeble but here as in other instances nature has adapted the means to the end proposed. What proofs are there that the circulation is carried on in the manner I have described? We know it from the direction of the valves of the heart of those of the arteries and veins and from other circumstances of no less conclusive character. In a wounded artery the blood flows from that part which is nearest to the heart and in an opposite direction. When a vein has been opened and compressed by a ligature the arteries swell above the vein. Examined by powerful magnifying glasses the circulation may be traced in the Salamander and in some other animals. By the ancients it was supposed that when the arteries contained air only the veins served as the reservoir of blood which was distributed during the day and returned during the night. They compared the circulation to the waves of the Euripus whose currents according to the poets run in opposite directions. About the middle of the 16th century there was something like an approach to the discovery

of the circulation of the blood in the lungs though little was known relative to its precise nature. About the year 1619 when Harvey first proclaimed his discovery so little were the minds of Physiologists prepared for such an event that it burst upon them as an entire novelty. We should be surprised that so obvious a circumstance remained so long undiscovered did we not recollect many parallel instances. Mankind from the beginning of the world had been accustomed to see the fall of an apple but for the genius of Newton was reserved the discovery of the cause. Truth perhaps in all sciences lies nearer the surface and is less disguised than the pride of Learning permits us to suspect.

Lecture 17th.

Nutrition.

It was stated by me on a former occasion, that the living body undergoes the process of alternate destruction and renovation. This evolution is so rapid and complete, that according to some physiologists every part of the animal is renewed once in seven years; according to others, once in three years; others say in a few months, and some even state the time to be fifty-six days. Estimations of this kind are unavoidably wanting in accuracy because we have no correct data from which to calculate. But though there is much difference in the results, yet the general fact that such a change does take place, is not disputed. It may however be enquired If such a renovation take place, why do not small pox and some other similar diseases return? But to this it may be replied that the matter returned and formed by the absorbents, is precisely similar to that which was removed. In this process two powers are concerned. Of the functions of the lymphatics we have already copiously treated. We are next to enquire by what means the waste occasioned by these is replenished. This is accomplished by depositions of nutritive matter from the extremities of the arteries. But that you may comprehend more fully the nature of the subject before us, I will briefly recapitulate some of the principal circumstances which have been already detailed. The food having been mixed

with the saliva, and received into the stomach is there converted into chyme a soft uniform fluid of a grey colour in which the previous texture and properties of aliment are no longer distinguishable. The solvent power of the gastric liquor is the chief agent in the production of these changes. As soon as chyme enters the intestines it unites with the bilious pancreatic and intestinal fluids and the result is a white liquor which however various the ingredients from which it is elaborated exhibits under all circumstances an unity and similarity of appearance and composition. With the exact process of Chylification we are unacquainted. Be this as it may, the chyle is taken up by the lacteals undergoes some change in its passage is deposited in the circulation and incorporated with the blood. It next passes into the lungs and being exposed to the action of the atmospheric air undergoes those changes which have been previously noticed. But what office is the blood to perform in the animal economy? Every part of our structure is subject to waste either from accident the ravages of disease or the constant action of the absorbent system; materials are required to supply the loss thus effected. The bones demand phosphate of lime and gelatine; the muscles want fibrine and the cartilages and membranes require supplies of albumen. The operation by which these are extracted from the blood and appropriated to the supply of the various parts is denominated Nutrition.

How this is accomplished it is impossible even vaguely to conjecture. That it does exist however we have the most ample evidence. The union of fractured bones the filling up of ulcerated cavities the healing of wounds by the first intention, not to mention the phenomena of growth are facts which point out indisputably the existence of such a function as nutrition. All that we know with certainty is that the nutritive matter is taken from the blood and deposited wherever the exigence of the system requires it. But how it is done and why one set of vessels give out one species of matter another of the same apparent structure another species is at present a mystery and most probably will always continue so. To say it is owing to animal appetency is a mere truism. It has been said that growth is a mere addition of parts similar in their nature (Is not growth rather an extension of parts?) like the increase of stone. But the cases are very different; in a living state the new part possesses the peculiar organization of the part to which it is added becomes identified with it in structure and resembles it in configuration and general appearance. The power of moulding and perfecting the newly deposited matter is appropriated to the absorbents and this is not the least curious office belonging to these vessels. By the arteries the matter is often roughly and profusely deposited as in fractures and ulcers. The absorbents go to work, blunt the asperities and remove the redundance of callus in the bones and in ulcers level smooth and symmetrize the often rough and prominent cicatrices.

Conformable to what has been said it follows that nu=trition is a compound process in which the two oppo=site antagonizing powers are engaged. While the arteries are busy in receiving the absorbents are no less so in removing the various parts of the frame so as to make room for new depositions. Thus in process of time the body loses its ori=ginal composition and is completely regenerated. Besides the direct subserviency of the blood to nutrition it is also employed in furnishing the different secretions. But though the names are not the same there is no actual difference between the two processes. Nutrition in every sense of the word is truly a secretory process several other functions may be ranked under the same term. Digestion is nothing more than a secretion of chyle from the excremen=titious and faecal part of the aliment. Can we consider respiration in any other light where a secretion takes place from the venous blood? The body may indeed be considered as a laboratory under the superintendance of the vital prin=ciple in which the various functions of the animal econ=omy are constantly employed in compounding and decom=posing in effecting the synthesis and analysis of objects within the sphere of their actions all the results of which are so many secretions from the blood.

Secretion.

As the secreted fluids are distinguished by several properties peculiar to themselves, several attempts have been made at a classification. The ancients divided them into Secretions and Excretions but the circumstance of the latter being thrown out of the body does not at all invalidate their claim to being considered as secretions. Nor should the distinction be retained as it is apt to lead to erroneous conceptions of the process. Chemistry having detected a still greater variety of properties than the external qualities gave reason to apprehend an arrangement has been proposed on that foundation. But however convenient this may be to the cultivators of that science, we as Physiologists cannot acquiesce in such a distribution. By secretion we understand that process by which a fluid is produced different from the materials out of which it is formed. As the fluids are various according to the structure of the organs which elaborate them the most natural basis for a classification is here offered. Every secreted fluid has a peculiar appearance adapted to its functions. The least complex of these are certain membranes made up principally of a series of arteries and veins from the former of which ducts open. Such are the lining membranes of the stomach trachea nose fauces urethra &c, and also those parts which go under the denomination of secreting surfaces. Whether rectilineal arteries or such as run in a straight direction are alone adequate to secretion is a point yet to determine. It is however wholly unworthy of serious enquiry.

Generally the vessels designed for this purpose pursue a tortuous or irregular course and this structure is evidently intended to retard the circulation of the blood. The exhalent vessels of the skin and those of the serous membranes the former producing perspiration the latter watery effusions in the cavities of the body are perhaps exceptions. Yet these few must be considered as having undergone a secretory action as neither of them is to be found in the blood exactly of the same consistence as after it has been separated. The secretory organs next in simplicity are the mucous follicles. These are small holes to be found in the mucous membranes particularly of the nose containing a small quantity of sebaceous matter. They exist also at the root of the tongue and in other parts. As yet their structure has not been accurately described; they are generally considered as an imperfect species of glands consisting of a convolution of vessels. Probably their structure is very like that of the conglobate or lymphatic glands already described by Hewson. The more perfect secretions as bile semen &c. require more perfect organs for their elaboration. These are called Conglomerate glands (to distinguish them from the Conglobate which belong to the absorbent system) and constitute the great abdominal viscera as the liver kidneys and pancreas.

Much difference of opinion has existed relative to their structure. Malpighi maintained that their arteries terminated in cells of follicles in which the secretions

are deposited. But this was utterly denied by Ruysch who by injections proved that their substance was principally made up by convoluted arteries terminating in veins and sending off an excretory duct. The apparatus is now before us but as to the modus operandi the question is yet to be solved. Physiologists with regard to this have maintained various sentiments which have received a complexion from the prevailing opinions of the time in which they flourished. Thus during the predominance of chemistry in the schools of physiology they resorted to fermentation or something analogous as the process by which secretion was accomplished. When mechanical notions began to prevail secretion was explained on the principles of geometry. The size of the vessels the angles ramifications and a great deal more of such stuff equally pertinent and elucidatory were introduced into the discussion of the subject. It was supposed that certain vessels were adapted to the passage of particles of one shape others for those of another and thus did they attempt to explain the difference in the secreted fluids. Circular triangular and square particles were all drawn into the hypothesis. Another set of the geometrical school maintained that the arteries became so small as to allow only the fine parts of the blood to pass. Even now there are not wanting some who consider the glands as a species of strainer by which the finer parts are separated from the grosser as when a liquid is strained through a sieve. All explanations however on mechanical principles are done away by the fact that the products of secretion differ from

any thing which was to be found in the blood. It was alledged by the geometrical Physiologists that the particles of bile were prismatic, of the semen, orbicular; of the urine, square, and of the saliva, cubical. In the present rectified state of our knowledge we are enabled to come nearer the point We seek an explanation in the resources of chemistry operating under the laws of vitality. It may reasonably be supposed that little else can here be advanced than mere conjecture. Even this however if tolerably plausible is not to be neglected. The blood is an exceedingly compound substance consisting of carbon hydrogen nitrogen oxygen sulphur phosphorus and a minute portion of lime iron potash and soda. Whether all these substances are derived from without or whether they are created by the animal organs is a point not satisfactorily determined. The mode by which some are received into the body is very intelligible though for others there is no obvious source. The latter especially nitrogen phosphorus iron and lime as their presence cannot be otherwise accounted for must be elaborated in the body. The fact indeed as regards these three articles rests on demonstration; each of them have been detected in animals which have been kept on aliment that could not contain a particle of them. Abernethy took some seeds and having moistened them placed them in flannel and confined them under a receiver yet the plants produced from them contained substances entire=

-ly different from the water or contained air. But whatever may be the precise origin of these various ingredients in the blood no one doubts their existence nor is it less certain that these are the only sources whence the secretions are derived. It has been stated that all we know of the structure of glands is that they consist of a series of convoluted vessels. Now it is impossible to conceive how from the mere action of these vessels the blood should produce such a variety of results. It is not supposing too much to think that a play of chemical affinities takes place between the compound parts of the blood in secretion. By some it has been surmised that this happens between the substance of the glands and the elements of the blood. Nothing is more ridi=culous than such a supposition for a certain consequence would be a destruction of the glandular structure. Nor is it necessary to explain the phenomenon. Compounds are liable to decomposition. Exactly in proportion to the number of ingredients of which they consist a slight disturbance of their parts creates new combinations. The blood is a compound of this kind. It is presumable that in its passage through the circulation as well as in the glands it experiences a change in proportion to its ele=ments and this change is various according as the peculiar structure of the blood may be. Considering how numerous are the elements and how variously they may be situated with re=gard to one another may we not imagine an infinity of results. The different combinations into which the principles of the blood enter are brought about by the structure of the part through which it passes. Thus bile differs from semen because blood in passing through the

liver undergoes a change in the relative portions of its ele=
=ments different from that which takes place in its passage
through the testicle. How numerous are the substances
that may be produced from a combination of only two in=
gredients. Thus oxygen and nitrogen may be combined
so as to form atmospheric air. Nitrous oxyde nitric oxyde
nitrous acid and nitric acid. No two substances are less
alike than atmospheric air and aqua-fortis yet they are
composed of the same materials differing only in the pro=
=portion of their ingredients If these two gases produce such
various results from their combinations in various propor=
=tions what a vast number of articles however different and
even opposite in their qualities might be formed from so
complicated a fluid as the blood.

Lecture 18th.

The chemical theory which I mentioned in my last lecture affords a very simple and imposing view of the function of Secretion, but we should not therefore be induced too hastily to adopt it. That in the secretory organs a chemical action does take place in the mode I have described to you, cannot be denied. But as every attempt to imitate it out of the body has failed, we must suppose that there is some power in the vital principle. That such is the case is supported by the consideration of the immunity which the organ enjoys while the process of secretion is going on. The protection of every part of the digestive, assimilative, and secretory systems, is wholly independent of chemical laws. If this were not the case, as each of them is composed of the same elementary parts with the blood itself, they would come into the play of chemical affinities, and experience a change in their structures and composition, as in fact takes place after death. The stomach during life resists the action of the gastric juice by which after death it is frequently dissolved. Besides, secretions are altered in their nature, promoted, or retarded, by causes which act on the general system. Different states of mind also have great ascendency over the process. Thus fear increases the discharge of urine, anger acts powerfully on the liver, and effects no less striking result from an interception of nervous influence. Mr. Haighton has shewn that if nerves going to any secre-

tory organ be partially divided, its action is much diminished; and if they be completely cut through or destroyed, it entirely ceases to perform the function of secretion. The same result was obtained by Mr Philips and Mr. Brodie, whose recent experiments have ascertained that when an animal is decapitated, all the secretions are interrupted though the circulation be continued by artificially inflating the lungs. What is very curious, the blood in this instance exhibits all the changes usually effected by respiration, yet still there is nothing secreted and no evolution of animal temperature. Some Amphibiates were made in this way to live for 24 hours, and during the whole time exhibited the phenomena mentioned above. These facts led to the consideration of morbid secretions. Every practitioner has had occasion to observe the effects of certain articles of the Materia Medica in increasing, if not altering the secreted fluids, such as senna, mercury, squills, &c. Other causes so impress the body as to produce an entire change in the products. Common inflammation will often cause a secretion of pus from those parts which had before produced mucus or serum. Among circumstances of this nature I must not omit certain morbid secretions produced by specific contagions. This is very curious and worthy of the closest attention. What is more extraordinary than that a particle of virus inserted under the skin should produce so great a change as is observable in Hydrophobia, the vaccine and variolous pustules, and in syphilis? More than one hypothesis has been advanced to explain these phenomena. By the older chemists it was held that a particle of the virus enter=

-ing the circulation acts as a kind of ferment assimilating the whole mass of blood to its own nature. Need I inform you that this hypothesis is opposed by the whole tenour of the medical creed in which I was initiated. To my mind nothing is more clear than that every change in the fluids is produced through the intervention of the solids. Not the slightest proof exists that the fluids are contaminated by mixture with extraneous substances. Much less that in this way any influence is exerted over the secretions. A fact which overturns the whole doctrine is that no disease can be propagated by inoculation with the blood Many unsuccessful attempts have been made in this way to communicate the smallpox measles syphilis and hydrophobia. Even the blood and flesh of animals who died of canine madness have been eaten with im=punity. The fact is that the proximate cause of all secre=tions is the same. It consists in a certain condition of the vessels variously modified by impressions made on them by different articles from which results a change in the relative portion of the elements of the blood and consequently a new combination. Every variety in the secreted substances may be explained on this principle. The vessels which at one time secreted mucus at another will produce a morbid virus because they are differently excited. Thus it is in gonorrhœa with the vessels of the urethra. No less curious in its nature is the power which certain animals possess of secreting deleterious poisons.

In this class are many reptiles and insects especially the serpent and the spider. Others animals are distinguished by secretions peculiar to themselves. The Ant for instance pours out a fluid of a speci-fic acid nature. The Cuttle fish secretes a dark liquor and the skunk a urine exceedingly offensive. The two latter are intended as means of protection. The skunk by the horrid stench of his urine repels the attacks of many animals and the cuttle fish when pursued envelopes himself in a dark cloud which serves the double purpose of concealing himself and driving off his adversary by its offensive smell. As re-gards the human species the principal secretions are bile saliva lymph the pancreatic juice semen urine liquor of the prostrate gland milk gastric juice tears fat the mucous and serous fluids and synobial liquor, the menses and wa-tery exhalations. I shall not occupy your time with a chemical analysis of each of these fluids nor shall I more than I have already done describe the organs by which they are produced. Their is considerable uniformity in their visible structure and with their slight variations the Professor of Anatomy will make you acquainted. In dismissing the subject it will not be superfluous to remark that the production of bile is pe-culiar in one respect. While every other secretion takes place from arteries a vein is subservient to this. The liver like all other glands is supplied by an artery which ramifies through its whole substance but this is comparatively small and de-signed merely for the nourishment of the part as the bron-chial arteries nourish the lungs. Besides this there is another

vessel which takes blood to the liver called the Vena Portarum, and made up of veins which come from the abdominal viscera. By some writers it has been considered as an artery, yet it cannot come under that denomination as it carries venous blood. Both the Vena Portarum and the hepatic artery terminate in the hepatic veins which enter into the Vena Cava. The former also terminates in small cells in the liver whence small ducts arise and uniting convey the secreted bile to the alimentary canal. The preceding is an account of the circulation of the liver. As before stated it appears that bile is elaborated from the veins, but this is not universally the case. The veins of the abdominal viscera have been known to enter the vena cava separately without entering the Vena Portarum. Of this there is an example in the Hunterian Museum. On other occasions the vena portarum is formed but shuns the liver being directly inserted into the vena Cava. A case of this kind is mentioned by Abernethy and another is recorded in the medical and Physical Journal of London. These cases are sufficient to shew that the hepatic artery is capable under certain circumstances of secreting bile when circumstances call for such an action and also to illustrate that law of the animal economy by which when there is a deficiency of any organ the want is supplied by an assumption of the power in another. It has been a question whether the hepatic artery does not always assist the Vena

Portarum. In support of the sentiment it has been said that if the Hepatic vein be tied and the artery injected the Vena Portarum becomes distended. The fact is not clearly established and even if it were I see no reason to draw from it the conclusion that the artery is subservient to the function for as there is a connexion between the Vena Porta and the hepatic vein and between the latter and the hepatic artery the fluid injected into the artery might very well pass through its corresponding vein and reach the first mentioned vessel. There is no direct proof that the artery assists in the secretion and the supposition is opposed to the whole tenor of analogy. I have mentioned that the menses are a secretion. This is no new opinion, I have inculcated it ever since I was a teacher. Every other opinion is totally irreconciliable with facts. Many of the ancient crude notions respecting them have been totally discarded or are at least mentioned by none whose knowledge has kept pace with the improvements in Physiology. Nobody pretends at present to impute it to lunar influence to fermentation to the venereal appetite or to general plethora. A local congestion of blood in the uterus ending in haemorrhage is the only one of them which has many supporters. That at the period of menstruation there is an accumulation of blood in the uterus is not denied. But every other gland when called upon for any extraordinary exertion becomes the centre of fluxion towards which the current of blood is directed. This happens in all secretory organs but more particularly in those whose action is periodical. As all se=

=cretions are elaborated from the blood the effect above sta=
=ted is necessary to its formation in any quantity. So far local congestion is well founded. But were this the sole cause of the discharge, the menses would be pure blood, which they are admitted by all not to be. (See Chapman's edition of Richerand.) Every organ of secretion has its peculiar stimulus. That which cau=
=ses secretion in the uterus, is derived most probably from the ovaries. Whether these are imperfect from congenital organization, or from disease; or whether they are wanting from extirpation, the effect is the same. In such cases the menses never appear. In St. Thomas's Hospital in London there was a remarkable instance of the effects resulting from the want of ovaries. A woman who attended the house as a nurse, had a very masculine appearance. She was destitute of mammæ and had a remarkably narrow pelvis; after her death Mr. Astley Cooper examined her body; and found that the ovaries were wanting. The woman had never men=
=struated, and had always shewn an aversion to men. The experience of every practitioner must have taught him that when the ovaries are in a schirrous state, dropsical, or affected with other diseases, so as to af=
=fect their functions, the menstrual discharge is interrupt=
=ed or never occurs. Upon the whole, there can be no doubt but that the menses are a secretion, and that the ovaries supply the influence.

Lecture 19th.

Sensation.

In my former lectures I have given you some account of the functions by which the body is constantly undergoing growth and decay. I will next call your attention to that important one which connects us with the objects that surround us, and serves as a distinguishing mark between animals and vegetables. I allude to Sensation.

This belongs to that class of the functions called Animal functions, or which French physiologists have called functions of relation. It is this important endowment which constitutes the high prerogative of animal nature.

Contained within the cranium there is a large soft pultaceous mass, denominated the brain. It consists of three parts the Cerebrum, the Cerebellum, and the Medulla Oblongata. This last portion is continued down the vertebral canal, and therefore after passing out of the encephalon, is called Medulla Spinalis. From the brain and Medulla Spinalis proceed the organs of Sensation or nerves, which are attributed to every part of the body. (For a correct demonstration of these I refer you to the Professor of Anatomy.) The nerves are white medullary cords, supported or enwrapped by a membranous covering or Involucra, and pervading in a greater or less degree every part of the human system. It is true that some parts in a healthy state possess little or no sensibility, but their becoming

highly sensible when diseased, is a sufficient evidence of their being possessed of nerves. Among these are bone, cartilage, tendon, &c. Nerves are united in three ways. 1st. They are joined at acute angles, and pass off so as to form a network or plexus. 2nd. To form by several nerves a ganglion, which is sometimes called a distinct brain. 3rd. They anastamose.

Nerves are subservient to the mind, enable us to hold communication with the rest of the body, and connect us with the world. Much has been written on the utility and pleasures of the senses. They have exercised the powers of eloquence in an eminent degree. To them we owe all impressions whether painful or pleasing, and enjoy the faculty of reason, that grand characteristic which elevates man above all other beings. The senses have been divided into Internal and External, or those which hold communication with different parts of the body, and those which connect us with the world. The seat of vision is the eye; by this sense we are enabled to distinguish the varieties of colour and other appearances, by the influence of light on the Optic Nerve. Hearing enables us to distinguish sounds, or the vibrations of elastic bodies. Sounds differ according to the force and frequency of the vibrations by which they are produced. By Touch we discover the difference between hard and soft, rough and smooth bodies. By Smell we distinguish different odours; man possesses this sense, comparatively speaking, but slightly; dogs and

some of the inferior animals in a more eminent degree, and by insects it is still more highly enjoyed. The sense of taste resides in the tongue, and according to some, partially in the fauces. Doctor Wistar in his lectures, used to mention the case of a man in Edinburgh born without a tongue, who had not lost the sense of taste. In other cases the tongue has been extirpated in cancerous affections, and the sense of taste partially retained. By this sense we also judge of the qualities of aliments. The internal senses are hunger, thirst, nausea, desire for the evacuation of the faeces, urine, &c. The question here arises, whether in man there is a Sensorium Commune? It is evident that there must be some organ to take cognizance of external impressions, for if a part be injured we feel pain, but if the nerve be divided no sensation is felt. Also, if the spinal marrow be divided or injured by a dislocation of the vertebrae, no cognizance is taken of impressions made on parts dependent on the divided portion. Again, compressions of the brain partially or entirely destroy sense; of this several cases are on record. Boerhaave relates the case of a man born with an opening in his cranium who for a small sum would allow it to be compressed, which instantly destroyed all sense and motion. A similar case was related by Dr. Wistar of a man who was scalped by an Indian, remained insensible for six months, and afterwards when the depressed portion of bone was removed, recovered. Physiologists were next led to enquire in what part the Sensorium Commune was situated. Some attributed it to the Pia Mater, while others insisted that it was

in the ventricles of the brain. It is however, now believed to be the whole substance of the brain. To prove this, all the other parts of the system have been removed, and sensation continued. Soemerring advances examples of this. The eyes and lips will continue to give expression for some time after the head has been severed from the body. A gnashing of the teeth has also been observed, and even blushing. The biographer of Marie Antoinette states that a blush was distinctly seen on her face after her head was separated by the guillotine; owing to an insult offered by her executioner, who was said to have laid his hand on her neck and then on her breast just before he beheaded her. Her countenance assumed the expression which such an outrage on her dignity and delicacy was calculated to produce. To this Soemerring adds several cases of inferior animals which seem to be conclusive from analogy; and Boerhaave states that birds will flutter long after their heads have been removed. The cock, he says, will lie still and apparently dead and afterwards flutter for some time. From this circumstance it appears that a remnant of life and sensation remains in the body, which must have its source in the spinal marrow. The Turtle and Viper evince the same, and Dr Monro tells of a frog which was beheaded, and the next day the body on being touched jumped away. He also mentions the body of a Horned Beetle, which

lived for two days and continued to act as usual. Some species of the butterfly will fly about in the air for some time, and some flies are said even to copulate after losing the head. When worms are divided, each piece becomes a separate worm, as in some species of Mollusca; the fact is more visible as we descend to inferior animals, and in them it is more com=mon to all parts of the system. These speculative remarks, gentleman, you will probably think of little importance, but they should be attended to in some degree, as distinguish=ing between the science and the art of medicine; or in other words, as the great line of demarcation between the liber=ally educated physician and the mere empiric.

I have laid before you the facts and reasons which go to establish the conclusion that in the human species there is a Sensorium Commune, and that this is in the brain. I also stated that as we descend to the inferior animals we find it more and more distributed throughout the whole extent of the nervous system. For the proper communication of sensation there are certain pre-requisites Viz. 1st. That there must be an external impression. 2nd. That the sentient extremities of the nerves must be in a healthy condition. 3rd. That the body of the nerve holding communication between the part receiving the impression and the Sensorium Commune must also be in a sound and healthy condition. When all those are present, we are led to enquire next, what is the nature of ner=vous influence? It was supposed by Newton and followed by

Hartley that it was a vibration in the nerves similar to that of a cord. On this subject several preliminary observations are necessary. If we suppose this to be the doctrine of nervous influence, we are led to enquire 1st. Whether do impressions continue for some time after the cause is removed? 2ndly. How are they transmitted along the column of the nerves? To the first we must answer in the affirmative. The optic nerve will continue imprest with a colour after the object has passed away. If you cause different colours to pass in succession before the eye, the impression will be a compound of all, which will continue after they have been removed. A yellow following a blue, will produce the impression of a green. In like manner taste and odour, in particular cases, will continue after the agent ceases to act. Against the doctrine of vibration it has also been alledged that the very nature of the nerves is unsuited to such an action. Nor is it true, as asserted by some, that the operation is assisted by the investing membrane of the nerves. By Haller, who experimented on this subject with the aid of microscopes, not the least alteration could be discovered in the nerves on the application of external agents. To this we may add in the language of a physiological writer "that to produce vibrations the nerves must be in a state of tension". On the contrary we know them to be less elastic than any other part of the body. Besides, were this the case, the action of external agents would keep up a constant confusion

and discord throughout the nervous system. By most physiologists this doctrine is now rejected. Others have resorted to the supposition of some fluid contained in the nerves and thrown into action by certain stimuli. Newton supposed it to be an æther. But that it is a fluid secreted by the brain, and extending from thence throughout the nervous cords, seems to be a more rational opinion, nor is there much weight in the objections stated against this doctrine. It is not strengthened by the circumstance that the nerves have to a certain extent, independent action They possess energy by the application of certain stimuli, to carry on their natural functions after they have been severed by the knife and their connexion with the brain destroyed Plausible however, as this theory may appear, others have been suggested. It has been asked, Might not magnetism or electricity be so connected with some of the constituent parts of the body as to make them undergo unknown combinations? The supposition appears the more rational, when we consider that certain animals have the power of generating something very like electricity, as the Torpedo. Placed behind the gills of this animal are a number of small cells in which this faculty lies, which has been compared to a galvanic battery. These cells communicate with each other and with the lungs and brain, and have been compared to the nerves passing out of the brain. This supposition seems not very irrational. By Dr. Monro a number of experiments have been devised to establish the identity of the Galvanic and nervous influence. To me however the difference is very distinct. It

is my opinion that when galvanism has the effect of keep-
ing up the energy of divided nerves, it acts merely as a
stimulus and does not establish their identity. Of all the
before mentioned suppositions, the existence of a nervous
fluid would seem the most probable; but even this would
not at all assist in the explanation of the phenomena. To
no other agent than the vital action can it be attribu-
ted. All sensations are dependent upon sympathy, which
may be reckoned among the peculiarities of animated
nature. Whatever may be thought of this hypothesis, it
is to me more rational than either vibration or the ner-
vous fluid.

It has been supposed by Gall that there is a cer-
tain connexion between certain portions of the brain
and the external conformation; that certain individu-
al portions have their corresponding feature. He suppo-
sed that each faculty of the mind had its particular and
distinct seat in the brain, and that in proportion as the
seat of any faculty preponderated, so it protruded and
moulded its corresponding feature; and consequently the
faculty which preponderates in the brain, is expressed
on the exterior of the head. On this hypothesis depends
the whole science of Cranidogy.

In addition to former reasons in favour of the brain
being the seat of the Sensorium Commune, we observe
that the brain is always proportioned in size to the ex-
tent of intellect possessed. In man the brain is twenty

four times as large in proportion to his size as that of an ox. In able and strong minded men also, we observe that the brain is comparatively compact and solid; while in men possessed of great corporeal vigour, the intellect is diminished. The ancient sculptors gave to the statues of their gladiators muscular and athletic bodies, while the heads were small and the countenances dull and unmeaning. The same obtains in ferocious and vigorous animals; their brain is small, witness the lion, tyger, &c. It has been observed that men possessed of uncommon vigour of mind and brilliancy of genius, are generally of weak and delicate constitutions and extreme nervous irritability. The same remark will as a general rule apply to small men, while large and muscular men seldom possess a very eminent degree of intellect.§

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§It is worthy of remark that some of the most renowned heroes on record were men of small stature, as Alexander the Great, Julius Cæsar, and Napoleon Buonaparte; and that many of the most brilliant geniuses suffered under ill health or personal deformity, some under both. Homer and Milton were blind; Horace had an asthma, and Virgil a fistula lachrymalis. Pope was deformed, Dr Johnson's sight was imperfect and he was affected with convulsive movements of the limbs; while of the greatest living poets, one is said to be of almost dwarfish dimensions, and two others are lame.

Transcriber's Note.

Ever since the appearance of the memorable Treatise on the Human Understanding by Locke, the new-born infant has been considered without mind and compared to a blank sheet of paper. It was his opinion, and one which was most universally followed, that all knowledge was derived from the external senses. But besides these, it has been alledged by C and can hardly be doubted, that there are certain internal movements which have considerable agency. To what, for example, can we ascribe the infant's seizing the nipple and sucking a few hours after birth? This inherent principle which prompts to certain actions has been denominated Instinct, between which and reason there is a striking distinction; the former relating to the internal, the latter to the external impressions. It has been alledged that no knowledge can be explained on the principle of innate ideas. But it may be asked, why is the infant so full of intelligence? Its exigencies create certain sensations, as hunger, thirst, &c. which being conveyed to the Sensorium, give impulse of action. The brute creation furnishes a still better solution to the problem. It follows then, that the phrase "Nihil intellectu quod non fuit insensu," is incorrect.

It was admitted by Locke, but not by Hartley, that independent of external impressions there are certain intrinsic ideas and processes, and that the above

assertion is subject to the exception "Nisi ipse intellectus," which is applied to certain creative impressions carried to the Sensorium Commune, and again called forth to produce scope of intelligence. It might also be asked, why children possessing equal advantages should display such a striking difference. We find in the same family a great blockhead and a man of genius; equally educated, the difference is ascribed to the difference of susceptibility to receive and tenacity to retain impressions, or to some primordial conformation.

As yet I have spoken of nerves as subservient to the mind; we will now consider them in a physical point of view, or as subservient to digestion, respiration, secretion, animal temperature, absorption, &c. I will first say a few words on nervous Sympathy. We must acknowledge that we have no intelligence of its nature, though from the phenomena produced it's existence cannot be denied. We are forced to acknowledge, "Causa latet vis est notissima". Newton observed, "By impulse I mean any force by which the particles of matter are impelled towards each other;" in like manner we use the word sympathy. Those sympathies which prevail among the viscera are explained by the communication of the nerves of the spinal marrow. But there are others, in which so far as we can ascertain, there is no nervous communication. It has been asserted that the Sensorium Commune sometimes commits errors in the direction of the sensation, and thus produces delusive sympathies; as in Morbus Coxarius, neck of the bladder,

Ulnar nerve &c. It is said by Locke and Hartley, that different organs are affected by the same cause; this is called association and accounts for all the associated actions of the mind. Sometimes different parts are affected from their identity of structure, as in males the Parotid and testes; in females the Parotid and mammae. These phenomena are observed both in a morbid and healthy condition.

Sympathy is divided into Continuous, Contiguous, Remote, Direct, Reverse, and Illusive. There is a connexion throughout the system, by which all its parts are so linked as to constitute one whole; but there are particular parts which should be distinguished, as the stomach, brain, heart, lungs, &c. According to Haller the muscles possess a peculiar nervous influence which he calls Vis Insita, which is every where identical, but not excited by the same stimuli. Thus some act by volition, as the voluntary muscles; the heart by blood, the bladder by urine, the intestines by bile. But though these are the natural and appropriate stimuli, others will affect them. According to Legallois, the nervous influence in muscles was different as it sprung from the spinal marrow or brain, the former supplying the involuntary, and the latter the voluntary muscles. I believe that muscular influence is derived from the nerves; thus if the spinal marrow be cut, the muscles depending upon it will contract

and become paralyzed. Nervous influence is somehow dependent on the circulation; the muscles demand for their contraction a certain stimulus from that source. Boerhaave proved this by tying the Vena Cava, whereby the muscles dependent there-on became paralyzed.

Annies

Amies